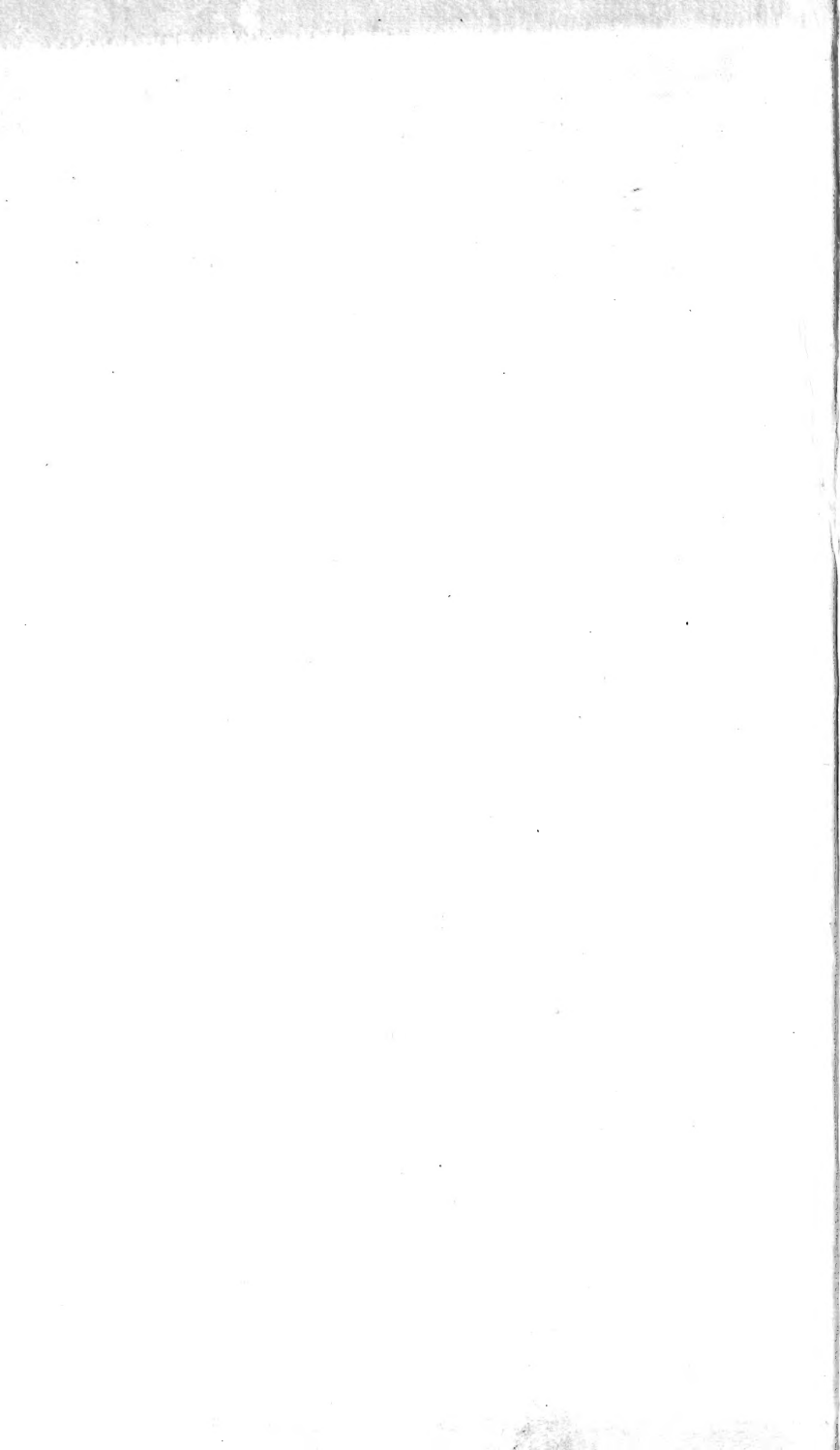
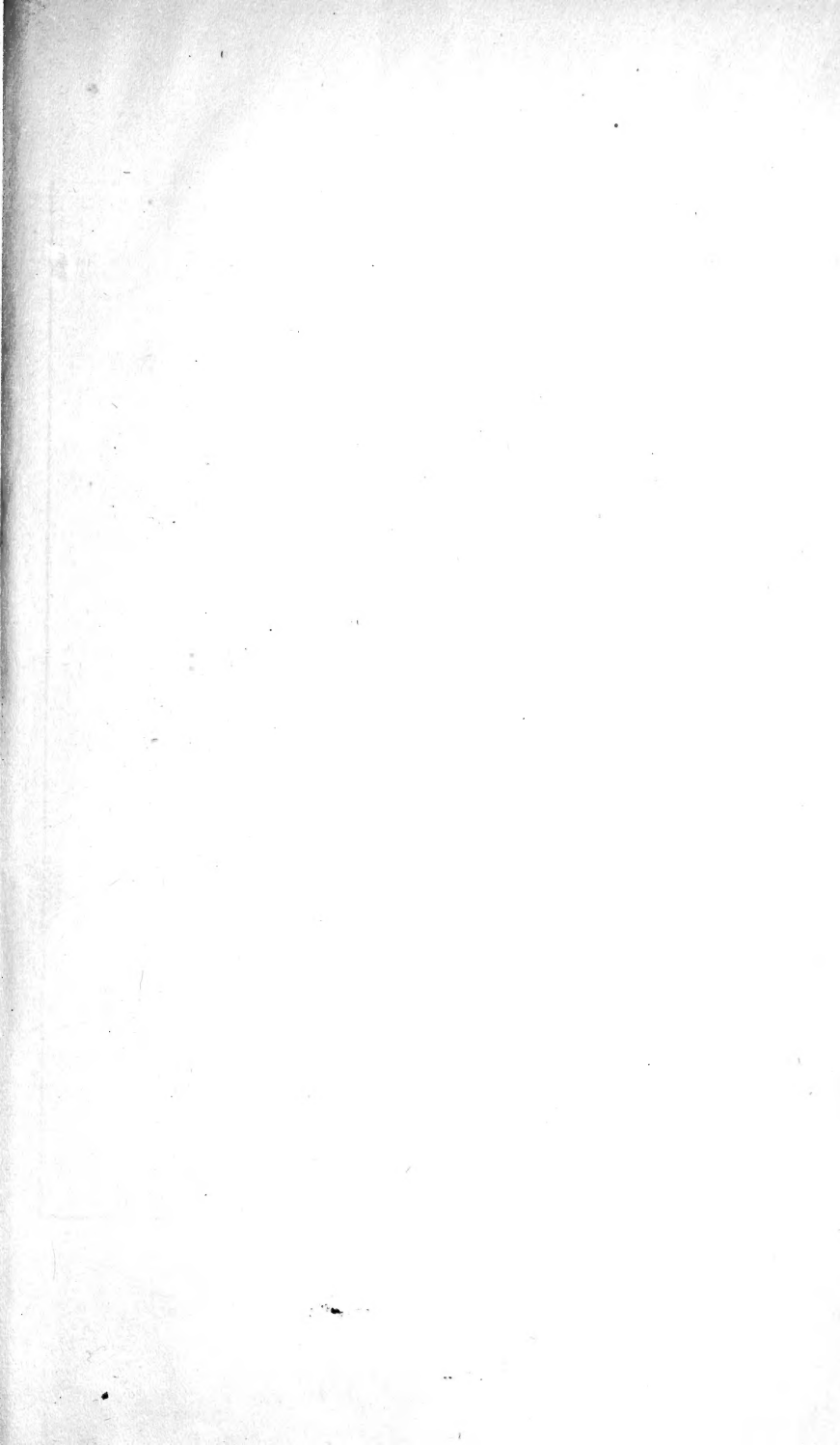


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PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

n.

From April 23, 1896, to February 18, 1897

VOL. LX.

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LONDON:

HARRISON AND SONS, ST. MARTIN'S LANE,

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Page 272, line 14 from bottom, *for* Dr. George Murray *read* Dr. John Murray.

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„ 298, „ 15 „ top, „ March, 1896, *read* February 26, 1896.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

April 23, 1896.

(Meeting for Discussion.)

Sir JOSEPH LISTER, Bart., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Paper was read for the purpose of opening the discussion:—

“On Colour Photography by the Interferential Method.” By G. LIPPMANN, Professor of Physics, Faculty of Sciences, Paris.
Communicated by Sir JOSEPH LISTER, Bart., P.R.S.

April 30, 1896.

Sir JOSEPH LISTER, Bart., President, in the Chair.

The Right Hon. Sir Richard Temple, Bart., a Member of Her Majesty's Most Honourable Privy Council, was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. “Note on Photographing Sources of Light with Monochromatic Rays.” By Captain W. DE W. ABNEY, C.B., D.C.L., F.R.S.

April 30, 1896—continued.

- II. "On the Determination of the Photometric Intensity of the Coronal Light during the Solar Eclipse of 16th April, 1893." By Captain W. DE W. ABNEY, C.B., D.C.L., F.R.S., and T. E. THORPE, LL.D., F.R.S.
- III. "The Total Eclipse of the Sun, April 16, 1893. Report and Discussion of the Observations relating to Solar Physics." By J. NORMAN LOCKYER, C.B., F.R.S.
- IV. "On some Palæolithic Implements found in Somaliland by Mr. H. W. Seton-Karr." By Sir JOHN EVANS, K.C.B., D.C.L., Treas. and V.P.R.S.

May 7, 1896.

Sir JOSEPH LISTER, Bart., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair as follows:—

Clarke, Lieut.-Col. Sir George Sydenham, R.E.	Murray, John, Ph.D.
Collie, J. Norman, Ph.D.	Pearson, Professor Karl, M.A.
Downing, Arthur Matthew Weld, D.Sc.	Stebbing, Rev. Thomas Roscoe Rede, M.A.
Elgar, Francis, LL.D.	Stewart, Professor Charles, M.R.C.S.
Gray, Professor Andrew, M.A.	Wilson, William E.
Hinde, George Jennings, Ph.D.	Woodward, Horace Bolingbroke, F.G.S.
Miers, Professor Henry Alexander, M.A.	Wynne, William Palmer, D.Sc.
Mott, Frederick Walker, M.D.	

The following Papers were read:—

- I. "On the Liqutation of certain Alloys of Gold." By E. MATTHEY. Communicated by Sir G. G. STOKES, F.R.S.
- II. "On the Occurrence of the Element Gallium in the Clay-Ironstone of the Cleveland District of Yorkshire. Preliminary Notice." By Professor HARTLEY, F.R.S., and H. RAMAGE.

May 7, 1896—continued.

- III. "The Electromotive Properties of *Malapterurus electricus*." By Professor GOTCH, F.R.S., and G. J. BURCH.
- IV. "The Occurrence of Nutritive Fat in the Human Placenta. Preliminary Communication." By Dr. T. W. EDEN. Communicated by Dr. PYE SMITH, F.R.S.

The Society adjourned over Ascension Day to Thursday, May 21.

May 21, 1896.

Sir JOSEPH LISTER, Bart., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

- I. "On the Changes produced in Magnetised Iron and Steel by cooling to the Temperature of Liquid Air." By Professor J. DEWAR, F.R.S., and Dr. J. A. FLEMING, F.R.S.
- II. "Note on the Larva and Post-larval Development of *Leucosolenia variabilis*, H. sp., with Remarks on the Development of other Asconidæ." By E. A. MINCHIN. Communicated by Professor LANKESTER, F.R.S.
- III. "Helium and Argon. Part III. Experiments which show the Inactivity of these Elements." By Professor RAMSAY, F.R.S., and Dr. J. NORMAN COLLIE.
- IV. "On the Amount of Argon and Helium contained in the Gas from the Bath Springs." By LORD RAYLEIGH, Sec. R.S.

The Society adjourned over the Whitsuntide Recess to Thursday, June 4.

June 4, 1896.

The Annual Meeting for the Election of Fellows was held this day.

Sir JOSEPH LISTER, Bart., President, in the Chair.

The Statutes relating to the election of Fellows having been read, Professor Bonney and Mr. Salvin were, with the consent of the Society, nominated Scrutators to assist the Secretaries in the examination of the balloting lists.

The votes of the Fellows present were collected, and the following Candidates were declared duly elected into the Society :—

Clarke, Lieut.-Col. Sir George Sydenham, R.E.	Murray, John, Ph.D.
Collie, J. Norman, Ph.D.	Pearson, Professor Karl, M.A.
Downing, Arthur Matthew Weld, D.Sc.	Stebbing, Rev. Thomas Roscoe Rede, M.A.
Elgar, Francis, LL.D.	Stewart, Professor Charles, M.R.C.S.
Gray, Professor Andrew, M.A.	Wilson, William E.
Hinde, George Jennings, Ph.D.	Woodward, Horace Bolingbroke, F.G.S.
Miers, Professor Henry Alexander, M.A.	Wynne, William Palmer, D.Sc.
Mott, Frederick Walker, M.D.	

Thanks were given to the Scrutators.

June 4, 1896.

Sir JOSEPH LISTER, Bart., President, in the Chair.

Professor Albert Gandry, who was elected a Foreign Member in 1895, was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

- I. "On the unknown Lines observed in the Spectra of certain Minerals." By J. NORMAN LOCKYER, C.B., F.R.S.
- II. "On the Electrical Resistivity of Bismuth at the Temperature of Liquid Air." By Professor J. DEWAR, F.R.S., and Dr. J. A. FLEMING, F.R.S.

June 4, 1896—continued.

- III. "On the Electrical Resistivity of pure Mercury at the Temperature of Liquid Air." By Professor J. DEWAR, F.R.S., and Dr. J. A. FLEMING, F.R.S.
- IV. "The Hysteresis of Iron and Steel in a rotating Magnetic Field." By Professor F. G. BAILY. Communicated by Professor LODGE, F.R.S.
- V. "Observations on Atmospheric Electricity at the Kew Observatory." By C. CHREE. Communicated by Professor G. CAREY FOSTER, F.R.S.

June 11, 1896.

Sir JOSEPH LISTER, Bart., President, in the Chair.

Dr. J. Norman Collie, Dr. A. M. W. Downing, Professor Andrew Gray, Dr. G. J. Hinde, Dr. F. W. Mott, Rev. T. R. R. Stebbing, Professor C. Stewart, Mr. W. E. Wilson, Mr. H. B. Woodward, and Dr. W. P. Wynne were admitted into the Society.

A congratulatory Address to Lord Kelvin, prepared for presentation to him on the occasion of the jubilee of his professoriate in the University of Glasgow, was read from the Chair and unanimously adopted.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

- I. "The Relation between the Refraction of the Elements and their Chemical Equivalents." By Dr. J. H. GLADSTONE, F.R.S.
 - II. "On the Magnetic Permeability and Hysteresis of Iron at Low Temperatures." By Dr. J. A. FLEMING, F.R.S., and Professor J. DEWAR, F.R.S.
 - III. "On certain Changes observed in the Dimensions of Parts of the Carapace of *Carcinus mœnas*." By H. THOMPSON. Communicated by Professor WELDON, F.R.S.
 - IV. "On the Relation between the Viscosity (Internal Friction) of Liquids and their Chemical Nature." By Dr. T. E. THORPE, F.R.S., and J. W. RODGER.
-

June 18, 1896.

Sir JOSEPH LISTER, Bart., President, in the Chair.

Lient.-Colonel Sir G. S. Clarke and Professor H. A. Miers were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

Sir J. W. Dawson exhibited new specimens of Carboniferous Batrachians.

An oral communication was made by Professor J. A. Fleming, F.R.S., on behalf of Professor Dewar and himself, to the following effect:—

In continuing our experiments on the electrical resistance of bismuth at low temperatures and in magnetic fields, by the aid of a powerful electro-magnet, kindly lent to us by Sir David Salomons, we have observed the fact that a wire of electrolytic bismuth, when cooled in liquid air to a temperature of -186° C., has its resistance increased more than forty-two times if it is at the same time transversely magnetised in a field of 14,000 units. The bismuth, when cooled in liquid air and thus magnetised, has its electrical resistance increased more than fifteen times, even when compared with its resistance at ordinary temperatures and not in a magnetic field. There is no reason to believe we have reached the limits of this increase. We reserve further details for a full communication to the Royal Society later.

The following Papers were read:—

- I. "Étude des Carbures Métalliques." By M. HENRI MOISSAN. Communicated by Professor RAMSAY, F.R.S.
- II. "On Fertilisation and the Segmentation of the Spore in Fucus." By J. B. FARMER and J. L. WILLIAMS. Communicated by Dr. D. H. SCOTT, F.R.S.
- III. "Complete Freezing-point Curves of Binary Alloys containing Silver or Copper together with another Metal." By C. T. HEYCOCK, F.R.S., and F. H. NEVILLE.
- IV. "Note on the Radius of Curvature of a Cutting Edge." By A. MALLOCK. Communicated by LORD KELVIN, F.R.S.
- V. "A Magnetic Detector of Electrical Waves and some of its Applications." By E. RUTHERFORD. Communicated by Professor J. J. THOMSON, F.R.S.

June 18, 1896—continued.

- VI. "Experimental Proof of van't Hoff's Constant, Dalton's Law, &c., in very dilute Solutions." By Dr. MEYER WILDERMANN. Communicated by Professor FITZGERALD, F.R.S.
- VII. "On the Determination of the Wave-length of Electric Radiation by Diffraction Gratings." By J. C. BOSE. Communicated by LORD RAYLEIGH, Sec. R.S.
- VIII. "The Effects of a strong Magnetic Field upon Electric Discharges in Vacuo." By A. A. C. SWINTON. Communicated by LORD KELVIN, F.R.S.
- IX. "On the Structure of Metals, its Origin and Changes." By M. F. OSMOND and Professor ROBERTS-AUSTEN, C.B., F.R.S.
- X. "Magnetisation of Liquids." By JOHN S. TOWNSEND. Communicated by Professor J. J. THOMSON, F.R.S.
- XI. "Selective Absorption of Röntgen Rays." By J. A. McCLELLAND. Communicated by Professor J. J. THOMSON, F.R.S.
- XII. "On the Determination of Freezing Points." By J. A. HARKER, D.Sc. Communicated by Professor SCHUSTER, F.R.S.
- XIII. "The Menstruation and Ovulation of *Macacus rhesus*; with Observations on the Changes undergone by the discharged Follicle. Part II." By WALTER HEAPE. Communicated by Dr. M. FOSTER, Sec. R.S.
- XIV. "Phenomena resulting from Interruption of Afferent and Efferent Tracts of the Cerebellum." By Dr. J. S. RISIEN RUSSELL. Communicated by Professor V. HORSLEY, F.R.S.

The Society adjourned over the Long Vacation to Thursday, November 19.

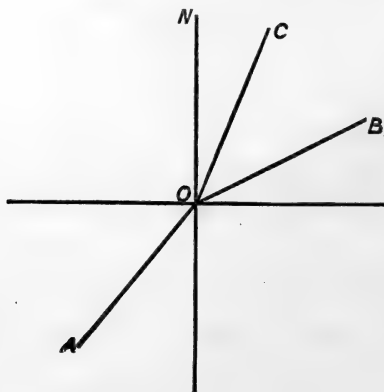
"Angular Measurement of Optic Axial Emergences." By WILLIAM JACKSON POPE. Communicated by Professor ARMSTRONG, F.R.S. Received February 7,—Read March 19, 1896.

Crystals belonging to the monoclinic or anorthic systems are rarely obtained in which the optical orientation is such that a large crystal face is so nearly perpendicular to a bisectrix that the apparent optic axial angle as observed in air can be directly measured by means of the ordinary Fuess apparatus. It thus becomes

necessary to first grind plates of known orientation for optical examination; this latter operation is by no means easily performed, especially in the case of brittle organic substances. Very usually, however, crystals belonging to the biaxial systems are obtained in which an optic axis apparently emerges into air through a particular face; in these cases the accurate measurement of the angle between the apparent direction in air of the optic axis and the normal to the crystal plate becomes an important element in the determination of the optical constants of the crystal.

The ordinary method of determining this angle is a direct one; the crystal is adjusted in the optic axial angle apparatus and a reading is taken for the above emergence, after the position of the normal to the plate has been found by reflecting a beam of light down the telescope tube and turning the crystal until the shadow and reflected image of the crosswires coincide; the angular difference between the two readings is then the required apparent angle of emergence into air. This method of finding the position of the normal is, however, very tedious, and, unless the crystal plate possesses a highly polished surface, very inaccurate.

To remedy these defects a method has been devised of indirectly determining this angle by calculating it from the angle through which the optic axis is apparently refracted by an oil of high refractive index. The crystal is mounted and adjusted in the optic axial angle apparatus in the ordinary way, and a reading is taken for the optic axial emergence in air; a parallel-sided glass cell containing α -bromonaphthalene or some other highly refractive liquid is then raised until it surrounds the crystal, and a second reading is taken of the apparent emergence of the optic axis. From the difference between these two angular readings the angle of emergence into air can be calculated, if the index of refraction of the oil is known.



In the figure, OA is an optic axial direction in the crystal, OB is the direction of optic axial emergence into air, and OC is the direction of emergence into a liquid of refractive index μ ; ON is the normal to the crystal plate. Then α , the angle of emergence into air, is NOB, whilst θ , the angle of emergence into the liquid is NOC and $\sin \alpha / \sin \theta = \mu$; it is required to calculate the angle α , from the observed value of $\alpha - \theta$.

Then, since $\sin \alpha / \sin \theta = \mu$,

$$\begin{aligned} \frac{1}{\mu} &= \frac{\sin \{\alpha - (\alpha - \theta)\}}{\sin \alpha} \\ &= \frac{\sin \alpha \cos (\alpha - \theta) - \cos \alpha \sin (\alpha - \theta)}{\sin \alpha} \\ &= \cos (\alpha - \theta) - \cot \alpha \sin (\alpha - \theta) \end{aligned}$$

and $\cot \alpha = \cot (\alpha - \theta) - \frac{1}{\mu \sin (\alpha - \theta)} \dots \dots \dots (1)$

Or again, since $\sin \alpha / \sin \theta = \mu$,

$$\begin{aligned} \frac{\mu + 1}{\mu - 1} &= \frac{\sin \alpha + \sin \theta}{\sin \alpha - \sin \theta} \\ &= \frac{\sin \frac{1}{2}(\alpha + \theta) \cos \frac{1}{2}(\alpha - \theta)}{\sin \frac{1}{2}(\alpha - \theta) \cos \frac{1}{2}(\alpha + \theta)}, \end{aligned}$$

whence $\tan \frac{\alpha + \theta}{2} = \frac{\mu + 1}{\mu - 1} \tan \frac{\alpha - \theta}{2} \dots \dots \dots (2)$

a form more convenient than (1) for logarithmic calculation.

To test the accuracy of the method, measurements have been made on biaxial plates of different optical properties, liquids of various refractive indices being used. The index of refraction of the liquid employed is conveniently determined with the Pulfrich refractometer; the refraction is so affected by differences of temperature and of purity that it is necessary to determine it for the liquid as actually used; the liquid does not need to be specially purified. The measurements given in the two appended tables were made on plates of topaz, each of them cut perpendicularly to the acute bisectrix. By measurement of the optic axial angles, the apparent emergences into air for sodium light were found to be $53^\circ 24'$ and $54^\circ 42'$, respectively.

These two sets of measurements suffice to show that the method possesses very considerable accuracy, although the values of $\alpha - \theta$ measured are not very large; the numbers also seem to indicate

TABLE I.—Plate with angle of emergence in air $53^{\circ} 24'$.

Liquid.	μ_D .	$\alpha - \theta$.	α .	Δ .
Carbon bisulphide.....	1.6473	$24^{\circ} 15'$	$53^{\circ} 26'$	+ $2'$
α -Bromonaphthalene....	1.5341	21 50	$53^{\circ} 22\frac{1}{2}$	- $1\frac{1}{2}$
Benzene	1.4970	21 0	$53^{\circ} 27\frac{1}{2}$	+ $3\frac{1}{2}$
Turpentine.....	1.4726	20 20	$53^{\circ} 20\frac{1}{2}$	- $3\frac{1}{2}$
Olive oil	1.4673	20 12	$53^{\circ} 21$	- 3
Glycerol	1.4634	20 10	$53^{\circ} 28\frac{1}{2}$	+ $4\frac{1}{2}$
Chloroform	1.4439	19 35	$53^{\circ} 19\frac{1}{2}$	- $4\frac{1}{2}$
Alcohol.....	1.3561	17 2	$53^{\circ} 15$	- 9
Water....	1.3327	16 21	$53^{\circ} 22$	- 2

TABLE II.—Plate with angle of emergence in air $54^{\circ} 42'$.

Liquid.	μ_D .	$\alpha - \theta$.	α .	Δ .
Carbon bisulphide.....	1.6473	$24^{\circ} 58'$	$54^{\circ} 38\frac{1}{2}'$	- $3\frac{1}{2}'$
α -Bromonaphthalene....	1.5341	22 35	$54^{\circ} 44\frac{1}{2}$	+ $2\frac{1}{2}$
Benzene.....	1.4970	21 41	$54^{\circ} 44\frac{1}{2}$	+ $2\frac{1}{2}$
Turpentine.....	1.4726	21 0	$54^{\circ} 37$	- 5
Olive oil	1.4673	20 56	$54^{\circ} 45$	+ 3
Glycerol	1.4634	20 51	$54^{\circ} 47\frac{1}{2}$	+ $5\frac{1}{2}$
Chloroform.....	1.4439	20 17	$54^{\circ} 42$	0
Alcohol.....	1.3561	17 45	$54^{\circ} 48\frac{1}{2}$	+ $6\frac{1}{2}$
Water.....	1.3327	16 54	$54^{\circ} 37$	- 5

that, as would of course be expected, the most accurate results are obtained with liquids of high refractive index, which give comparatively large values of $\alpha - \theta$. By determining the values of $\alpha - \theta$ for each of two optic axes of a given crystal plate, it can easily be ascertained with what amount of accuracy the plate has been cut perpendicularly to the bisectrix.

The principle of the method here described may very possibly be advantageously employed in other branches of optical investigation.

“On Colour Photography by the Interferential Method.” By G. LIPPMANN, Professor of Physics, Faculty of Sciences, Paris. Communicated by Sir JOSEPH LISTER, Bart., P.R.S.
Received April 14,—Read April 23, 1896.

Colour photographs of the spectrum, or of any other object, are obtained by the following method. A *transparent* photographic film of any kind has to be placed in contact with a metallic *mirror* during

exposure. It is then developed and fixed by the usual means employed in photography, the result being a *fixed* colour photograph visible by reflected light.

The mirror is easily formed by means of mercury. The glass plate carrying the film being inclosed in a camera slide, a quantum of mercury is allowed to flow in from a small reservoir and fill the back part of the slide, which is made mercury-tight. The plate is turned with its glass side towards the objective, the sensitised film touching the layer of mercury. After exposure, the mercury is allowed to flow back into its reservoir, and the plate taken out for development.

The only two conditions necessary for obtaining colour, transparency of the film and the presence of a mirror during exposure, are physical conditions. The chemical nature of the photographic layer has only secondary importance; any substance capable of giving, by means of an appropriate development, a fixed colourless photograph, is found to give, when backed by the mirror, a fixed colour photograph.

We may take, for instance, as a sensitive film, a layer of albumen-iodide of silver, with an acid developer; or a layer of gelatino-bromide of silver, with pyrogallie acid, or with amidol, as developers. Cyanide or bromide of potassium may be as usual employed for fixing the image. In a word, the technics of ordinary photography remain unchanged. Even the secondary processes of intensification and of isochromatisation are employed with full success for colour photography.

The photographic films commonly in use are found to be opaque, and formed, in fact, by grains of light-sensitive matter mechanically imprisoned by a substratum of gelatine, albumen, and collodion. What is here wanted is a fully transparent film, the light-sensitive matter pervading the whole of the neutral substratum. How can such a transparent film be realised? This question remained insoluble to me for many years, so that I was debarred trying the above method when I first thought of it. The difficulty, however, is simply solved by the following remark. It is well known that the precipitation of a metallic compound, such as bromide of silver, does not take place in the presence of an organic colloid, such as albumen, gelatine, or collodion. In reality, the metallic compound is formed, but remains invisible; it is retained in a transparent modification by the organic substances. We have only, therefore, to prepare the films in the usual way, but with a stronger proportion of the organic substratum; the result is a transparent film. By mixing, for instance, a gelatinous solution of nitrate of silver with a gelatinous solution of bromide of potassium, no precipitate is formed, and the result is a transparent film of dry gelatine containing 15 and even 30 per cent. of the weight of bromide of silver.

The colours reflected by the film are due to interference: they are of the same kind as those reflected by soap bubbles or by Newton's rings. When a ray of definite wave-length falls on the sensitive plate, it is during exposure reflected back by the mirror, and then gives rise to a set of standing waves in the interior of the film, the distance between two successive loops being equal to half the wave-length of the luminous ray. This system of standing waves impresses its periodical structure on the film. The photographic deposit, therefore, takes the form of a grating, a continuous grating, perfectly adapted for reflecting the particular luminous ray which has given it birth.

This theory can be subjected to experimental proof. If we examine a photograph of the spectrum, or any other object by white light, we observe the following facts. (1.) Colours are seen in the direction of specular reflection, and are invisible in every other direction. (2.) The colours change with the incidence; the red changing successively to green, blue, and violet, when the incidence grows more oblique. The whole image of the spectrum is displaced, and gradually passes into the infra-red region. (3.) If the film be gradually moistened, the colour changes in the opposite direction, from violet to red. This phenomenon is due to the swelling up of the gelatine or albumen, causing the intervals between the elements of the grating to become larger. The smaller intervals, corresponding to violet and blue light, gradually swell up to the values proper to red and infra-red waves. A photograph immersed in water loses all its colours, these appearing again during the process of drying. For the same reason, a freshly prepared plate has to be dried before the correct colours can be finally seen.

We have now to consider the case of compound colours, and to generalise the former theory, which is only applicable to the action of simple rays. I beg to subjoin an abstract of this generalised theory. It will be seen that if a compound ray of definite composition impresses the plate, it gives rise during exposure to a definite set of standing waves, which impress their structure on the film, and impart to the photographic deposit a corresponding definite form. Though very complex, this can be described as made up of a number of elementary gratings, each corresponding to one of the simple rays which contribute the impressing light. When examined by white light, the reflected ray is shown to have the same composition as the impressing ray; white light, for instance, imparts to the photographic deposit such a structure that it is adapted to reflect white light.

The only *a priori* condition for the correct rendering of compound rays, is a correct isochromatisation of the film. This, again, can be practically effected by known processes, such as have been indicated by E. Becquerel, Vogel, Captain Abney, and others.

As a verification of this theory, I beg leave to project on the screen a series of colour photographs, representing natural objects: pictures on stained glass, landscapes from nature, flowers, and a portrait from life. Every colour in nature, including white, and the delicate hue of the human complexion, is thus shown to be reflected by a correctly developed photographic film.

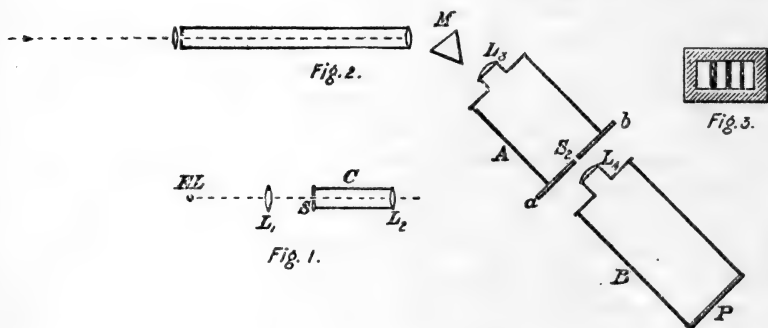
It is to be remarked that, as in the case of the spectrum, the colours are visible only in the direction of specular reflection. If I had tried to touch up these photographs by means of water colours or other pigments, these would be made apparent by slightly turning the photograph; these pigments remaining visible under every incidence, they would thus be seen to stand out on a colourless background. Thus the touching up or falsifying by hand of a colour photograph is happily made impossible.

“Note on Photographing Sources of Light with Monochromatic Rays.” By Captain W. DE W. ABNEY, C.B., D.C.L., F.R.S. Received March 31,—Read April 30, 1896.

In a paper “On the Production of Monochromatic Light,” communicated to the Physical Society, and read on the 27th June, 1885, and which appears in the ‘Philosophical Magazine’ for August in that same year, I stated that by the apparatus then described a monochromatic image of the sun could be thrown upon the screen. In the same periodical for June of the same year, Lord Rayleigh described a plan for obtaining a monochromatic image of an external object, in which a concave lens was placed behind the slit of a spectroscopic to produce an image of the object in monochromatic colour, the object being viewed through an aperture placed in the spectrum produced by the apparatus. I had been working independently at the subject at the same time, and my object was to get an image on a screen or photographic plate rather than to use the apparatus for visual observation. When a lens is placed behind the spectrum in the manner described in the paper above referred to, a white image of the prism can be obtained on a screen placed at some distance from the lens, and the size of the image can be increased or diminished according to the focal length of the lens, and its distance from the spectrum. Evidently, then, if an image of a luminous object can be cast on the surface of the prism, and a slit be placed in the spectrum, the image of the luminous object will be seen of the colour of the light passing through the slit. There are devices adopted at the present time for photographing the sun with light of various wave-lengths, but, as far as I am aware, they depend upon moving the image of the sun across the slit of the spectroscopic, the

plate moving across the slit in the spectrum at the requisite rate for the various impressions made by the different parts of the sun's image to coalesce. It had struck me some time since that the method thus indicated nearly eleven years ago might be more convenient than that adopted, but the time I had at my disposal prevented my carrying out a continuation of my experiments. Recently I have had occasion to take up this subject for a rather different purpose, and as the method seems to have been untried, I give it in more detail than I did then.

My investigation called for a determination of the proportions of various rays emitted by the various parts of the carbon of the positive and negative poles of an electric arc light, and for this purpose the system of forming monochromatic images was found to be useful. The points of the electric light EL (fig. 1) were placed so that a beam



of light passed through the slit S of the collimator on to the centre of the collimating lens L_2 . A convex lens L_1 of shorter focus than L_2 was placed in the path of the rays, and so adjusted that a real image of the poles was formed on L_2 . These passed through the lens L_2 as nearly parallel rays and struck upon the prism, and then passed through the remainder of the apparatus as sketched in fig. 2, where M is the prism, L_3 a lens to bring the rays to a focus as a spectrum on ab after passing through a camera, A . L_4 is a lens, shown in the figure connected with a camera, B , which brings the image of the prism and the bright image cast on it to a focus at P . By placing a slit S_2 in the spectrum, the image cast on P will be as monochromatic as the light coming through the slit. L_1 should be of such a focal length that it should be as near the slit as possible. With this arrangement it is very curious to watch the variations in the brightness of the arc and of the flame which accompanies the movement of the slit through the spectrum, and as each variation can be photographed on a Cadett polychromatic photographic plate, we can obtain records of all that is

occurring. Further, by using strips of lenses cut out at suitable distances from the axes (fig. 3), images of various colours can be placed side by side upon P, since a slit may be placed in the spectrum opposite each such strip of lens. Incidentally, I may mention that investigations into the cause of the variable nature of different flames can be carried out by this plan.

For solar work, a long collimator appears to be a necessity, but the aperture need not be large. Suppose we determine to have an image of the sun on P (fig. 2) of 2 in. diameter, the image on M need not be more than 1 in. at most. For this purpose we must have a collimator 10 ft. long. Two lenses of this focal length can be fixed one at each end, and a slit in front of that lens which is presented to the sun's rays. The arrangements followed will be the same as those given for the electric light. There appears no difficulty in producing a monochromatic image of almost any size if the collimator be sufficiently long and the face of the prism sufficiently large to take in the whole of the image cast on it.*

I have replaced the prism by flat refraction gratings with most satisfactory results. The gratings I employed had about 6,000 and 12,000 lines to the inch. The images were sharply defined, but, of course, weaker than when the prism was employed. For solar work this should not be an objection, since there is plenty of light to work with.

I show some pictures taken by the plan I have described. For my purpose the images are sufficiently sharp, although simple uncorrected lenses have been employed.

“On the Determination of the Photometric Intensity of the Coronal Light during the Solar Eclipse of 16th April, 1893.” By Captain W. DE W. ABNEY, C.B., D.C.L., F.R.S., and T. E. THORPE, LL.D., F.R.S. Received April 14,—Read April 30, 1896.

(Abstract.)

In this paper the authors give the results of the measurements of the intensity of the light of the corona, as observed at Fundium in Senegal, on the occasion of the solar eclipse of April 16th, 1893. The methods employed by them were practically identical with those used at Grenada, in the West Indies, during the eclipse of 1886, an account of which is given in the ‘Phil. Trans.’ A, 1889,

* It should be mentioned that to minimise diffraction the slits should be used fairly wide. Hence a long collimator such as described and a good dispersion will be necessary to obtain the best definition of the sun's image.—April 30.

p. 363, with certain slight modifications suggested by their experience on that occasion. Two sets of observations were made: the first with a photometer equatorially mounted, and designed to measure the comparative brightness of the corona at different distances from the moon's limb, and the second with an instrument arranged to measure the total brightness of the corona, excluding as far as possible the sky effect. In both cases the principle of photometry was that of Bunsen, the intensity of the coronal light being compared with that of a standard glow-lamp, according to the method of Abney and Festing.

The measurements with the equatorial photometer were made by Dr. Thorpe, assisted by Mr. P. L. Gray, B.Sc., those with the second or integrating instrument were made by Mr. Jas. Forbes, jun., assisted by Mr. Willoughby, of H.M.S. "Alecto."

The mean of ten concordant readings with the integrating photometer reduced to values of light intensity and expressed in Siemens' units was 0·026.

The measurements with the equatorial photometer show that the visual brightness of the corona of the 1893 eclipse varied within comparatively wide limits, and that, at all events close to the moon's limb, there was marked variation in local intensity. If the several values taken in the direction of the poles and equator are grouped as in the former paper (*loc. cit.*), they are found to afford a curve almost identical in character with that already given, showing that the diminution in intensity from the moon's limb outwards is less rapid than accords with the law of inverse squares.

The results are as follows :—

Distances in solar semi-diameters.	Photometric Intensity.		
	Observed.		Law of inverse squares.
	1893.	1886.	
1·6	0·060	0·066	0·066
2·0	0·048	0·053	0·042
2·4	0·038	0·043	0·029
2·8	0·030	0·034	0·022
3·2	0·024	0·026	0·016
3·4	0·018	0·021	0·013

These numbers would appear to show that the actual brightness of the corona was probably not very dissimilar at the two eclipses, the slight apparent diminution observed during the 1893 eclipse being,

in all probability, due to the haze, or opalescence, in the air which prevailed at the time. This haze, caused more by suspended and finely divided solid matter than by precipitated moisture, undoubtedly contributed to the general sky-illumination at the time of totality. The actual gloom during this phase of the eclipse at Fundium was certainly much less than at Grenada in 1886. It must not be forgotten, however, that the altitude of the sun was very different on the two occasions. At Grenada it was only about 19° : the amount of cloud was from seven to eight (overcast = 10) at the time of totality, and much of the cloud was in the neighbourhood of the sun: whereas at Fundium the sun's altitude was 52° , and the sky was of a bluish-grey colour and practically free from cloud.

The effect of these different conditions in the sky in the neighbourhood of the disc is seen in Mr. Forbes' measurements when compared with those of Lieutenant Douglas, at Grenada. The ten fairly concordant observations at Fundium give, as already stated, an average value of 0.026 Siemens units at 1 ft. from the screen; and the value observed by Lieutenant Douglas, 15 seconds after totality, with the same photometer, although with a different lamp and galvanometer, was 0.0197 light units.

“The Total Eclipse of the Sun, April 16, 1893. Report and Discussion of the Observations relating to Solar Physics.”

By J. NORMAN LOCKYER, C.B., F.R.S. Received April 17,

—Read April 30, 1896.

(Abstract.)

The memoir first gives reports by Mr. Fowler and Mr. Shackleton as to the circumstances under which photographs of the spectra of the eclipsed sun were taken with prismatic cameras in West Africa and Brazil respectively on April 16, 1893. These are followed by a detailed description of the phenomena recorded, and a discussion of the method employed in dealing with the photographs. The coronal spectrum and the question of its possible variation, and the wavelengths of the lines recorded in the spectra of the chromosphere and prominences, are next studied.

Finally, the loci of absorption in the sun's atmosphere are considered.

The inquiry into the chemical origins of the chromospheric and prominence lines is reserved for a subsequent memoir.

The general conclusions which have been arrived at are as follows:—

(1) With the prismatic camera, photographs may be obtained with

short exposures, so that the phenomena can be recorded at short intervals during the eclipse.

(2) The most intense images of the prominences are produced by the H and K radiations of calcium. Those depicted by the rays of hydrogen and helium are less intense, and do not reach to so great a height.

(3) The forms of the prominences photographed in monochromatic light (H and K), during the eclipse of 1893, do not differ sensibly from those photographed at the same time with the coronagraph.

(4) The undoubted spectrum of the corona in 1893 consisted of eight rings, including that due to 1474 K. The evidence that these belong to the corona is absolutely conclusive. It is probable that they are only represented by feeble lines in the Fraunhofer spectrum, if present at all.

(5) All the coronal rings recorded were most intense in the brightest coronal regions, near the sun's equator, as depicted by the coronagraph.

(6) The strongest coronal line, 1474 K, is not represented in the spectrum of the chromosphere and prominences, while H and K do not appear in the spectrum of the corona, although they are the most intense radiations in the prominences.

(7) A comparison of the results with those obtained in previous eclipses confirms the idea that 1474 K is brighter at the maximum than at the minimum sun-spot period.

(8) Hydrogen rings were not photographed in the coronal spectrum of 1893.

(9) D₃ was absent from the coronal spectrum of 1893, and reasons are given which suggest that its recorded appearance in 1882 was simply a photographic effect due to the unequal sensitiveness of the isochromatic plate employed.

(10) There is distinct evidence of periodic changes of the continuous spectrum of the corona.

(11) Many lines hitherto unrecorded in the chromosphere and prominences were photographed by the prismatic cameras.

(12) The preliminary investigation of the chemical origins of the chromosphere and prominence lines enables us to state generally that the chief lines are due to calcium, hydrogen, helium, strontium, iron, magnesium, manganese, barium, chromium, and aluminium. None of the lines appear to be due to nickel, cobalt, cadmium, tin, zinc, silicon, or carbon.

(13) The spectra of the chromosphere and prominences become more complex as the photosphere is approached.

(14) In passing from the chromosphere to the prominences, some lines become relatively brighter but others dimmer. The same line sometimes behaves differently in this respect in different prominences.

(15) The prominences must be fed from the outer parts of the solar atmosphere, since their spectra show lines which are absent from the spectrum of the chromosphere.

(16) The absence of the Fraunhofer lines from the integrated spectra of the solar surroundings and uneclipsed photosphere shortly after totality need not necessarily imply the existence of a reversing layer.

(17) The spectrum of the base of the sun's atmosphere, as recorded by the prismatic camera, contains only a small number of lines as compared with the Fraunhofer spectrum. Some of the strongest bright lines in the spectrum of the chromosphere are not represented by dark lines in the Fraunhofer spectrum, and some of the most intense Fraunhofer lines were not seen bright in the spectrum of the chromosphere. The so-called "reversing layer" is therefore incompetent to produce the Fraunhofer spectrum by its absorption.

(18) Some of the Fraunhofer lines are produced by absorption taking place in the chromosphere, while others are produced by absorption at higher levels.

(19) The eclipse work strengthens the view that chemical substances are dissociated at solar temperatures.

"On some Palæolithic Implements found in Somaliland by Mr. H. W. Seton-Karr." By Sir JOHN EVANS, K.C.B., D.C.L., Treas. and V.P.R.S. Received April 27,—Read April 30, 1896.

Although some account of his recent discoveries in Somaliland (tropical Africa) has already been given to the Anthropological Institute by Mr. Seton-Karr, and has been published in their Journal,* these discoveries seem to me to have so wide an interest, and such an important bearing on the question of the original home of the human race, that I venture to call the attention of this Society to them.

In the course of more than one visit to Somaliland for sporting purposes, Mr. Seton-Karr noticed, and brought home for examination, a number of worked flints, mostly of no great size, which he laid before the Anthropological Section of the British Association, at the meeting last year at Ipswich.† Although many of these specimens were broad flat flakes trimmed along the edges so as to be of the "le Moustier type" of M. Gabriel de Mortillet, and although the general *facies* of the collection was suggestive of the implements being of palæolithic age, they did not afford sufficient evidence to enable a satisfactory judgment to be formed whether they undoubtedly belonged to the palæolithic period.

* Vol. 25, p. 271.

† Report, 1895, p. 824.

Before returning to Somaliland, Mr. Seton-Karr visited my collections, and studied the various forms of implements found in the river-gravels and Pleistocene deposits in different parts of the world, so as to become familiar with their leading features; and on revisiting Somaliland during the past winter, he was fortunate enough to meet with a large number of specimens in form absolutely identical with some from the valley of the Somme and other places which he had seen in my collection.

Of this identity in form there can be no doubt, and though at present no fossil mammalian or other remains have been found with the implements, we need not hesitate in claiming them as palæolithic. They seem to be scattered all over the country, and to have been washed out of sandy or loamy deposits by the action of rain, or, in some instances, to have been laid bare by the wind. They appear also to occur most frequently in the neighbourhood of existing water-courses, which is at all events suggestive of the beds in which they occur having been in some manner the result of river-action. It is, however, at present premature to enlarge on the circumstances of their discovery. Their great interest consists in the identity of their forms with those of the implements found in the Pleistocene deposits of North Western Europe and elsewhere. Any one comparing the implements from such widely separated localities, the one with the other, must feel that if they have not been actually made by the same race of men, there must have been some contact of the closest kind between the races who manufactured implements of such identical forms. Those from Somaliland occur in both flint (much whitened and decomposed by exposure) and in quartzite, but the implements made from the two materials are almost indistinguishable in form. Those of lanceolate shape are most abundant, but the usual ovate and other forms are present in considerable numbers.

Turning westward from Somaliland we meet with flint implements of the same character found by Professor Flinders Petrie at a height of many hundred feet above the valley of the Nile. A few have been discovered in Northern Africa, they recur in the valley of the Manzanares in Spain, in some districts in Central Italy, and abound in the river-valleys of France and England. Turning eastward we encounter implements of analogous forms, one found by M. Chantre in the valley of the Euphrates, and many made of quartzite in the laterite deposits of India; while in Southern Africa almost similar types occur, though their age is somewhat uncertain.

That the cradle of the human family must have been situated in some part of the world where the climate was genial, and the means of subsistence readily obtained, seems almost self-evident; and that these discoveries in Somaliland may serve to elucidate the course by which human civilisation, such as it was, if not indeed the human

race, proceeded westward from its early home in the east is a fair subject for speculation. But, under any circumstances, this discovery aids in bridging over the interval between palæolithic man in Britain and in India, and adds another link to the chain of evidence by which the original cradle of the human family may eventually be identified, and tends to prove the unity of race between the inhabitants of Asia, Africa, and Europe, in Palæolithic times.

“On the Liqutation of certain Alloys of Gold.” By EDWARD MATTHEY, F.S.A., F.C.S., Assoc. R.S.M. Communicated by Sir G. G. STOKES, Bart., F.R.S. Received April 14,—
Read May 7, 1896.

The molecular distribution of the metals in alloys of gold and of metals of the platinum group has been described by me at some length, in a series of papers which have already been published by the Royal Society.* New interest in the subject has, however, arisen in connexion with the extraordinary development in various parts of the world especially in South Africa, of certain processes which are now employed for extracting gold from its ores. Their use has been attended with the introduction into this country of a series of alloys of gold and the base metals which have hitherto rarely been met with in metallurgical industry. The base metals associated with the gold in these cases are usually the very ordinary ones lead and zinc, but their presence in the gold has given rise to unexpected difficulties, as the distribution of the precious metal in the ingots which reach this country is so peculiar, that it is not possible to estimate the value of the ingots by taking the pieces of metal required for the assay, by any of the well-known methods now in use.

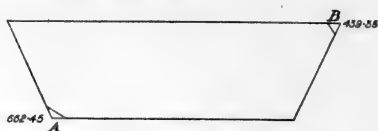
The grouping of the metal in these ingots presents much scientific as well as industrial interest, and the following is a brief statement of the facts which have been observed.

A. An ingot of gold weighing 3·545 kilograms was assayed with a view to subjecting it to the ordinary operation of refining. A piece of metal was, therefore, cut from the base of the ingot at the point marked A, and the following are the results of four assays made on this piece of metal:—

Gold 1	665·8
2	663·6
3	662·4
4	658·0
<hr/>	
Average	662·45

* ‘Phil. Trans.’ A, vol. 183, p. 629, 1892. ‘Roy. Soc. Proc.’ vol. 47, p. 180, 1890.

There was also 0·061 part of silver present in 1000 parts of the mass, the remainder being base alloy.



Another set of assays from the same ingot, but from the opposite end, at the point marked B, gave the following results:—

1	429.9		
2	459.5		
3	439.0		
4	429.0	Silver	0.071

Average 439.35

The difference in the amount of gold between the two opposite ends of the ingot was, therefore, no less than 223.10 parts in 1000. The base metal present was proved by analysis to be chiefly zinc, lead, and copper, as the following results will show on metal taken by a “dip,” *i.e.*, from the molten metal:—

Zinc	15.0
Lead	7.0
Copper	6.5
Iron	2.2
Nickel	2.0
Silver	7.0
Gold (by difference)	60.3
	<hr/>
	100.0

B. Another ingot of alloyed gold weighing 12.223 kilograms gave at different parts of the ingot the following results by assay:—



Four assays on a piece of metal cut at *a*—top of ingot—

	Gold.	Silver.
1	664.0	0.090
2	662.5	0.091
3	465.0	0.076
4	661.5	0.091

Three assays at *b*—bottom of ingot—

	Gold.	Silver.
1	332·5	0·181
2	652·0	0·095
3	410·5	0·057

And seven assays were made from a “dip,” viz.—

	Gold.	Silver.
1	622·0	—
2	574·4	0·072
3	653·5	0·011
4	623·2	—
5	580·0	0·138
6	603·3	—
7	562·3	—

Average of the whole number of the assays made	576·2	0·090
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It became evident, therefore, that the only method of determining the true quality of this ingot consisted in actually separating the gold and silver in mass, and this was effected by dissolving in nitrohydrochloric acid, the silver being recovered as chloride and reduced to metallic silver, and the gold precipitated by iron chloride as pure metallic gold.

The result of this operation yielded

Gold	7·504 kilograms.
Silver	0·928 „

which showed that the standard fineness of the ingot was

Gold	614·0
Silver	75·8

and its true value £1,028; while the value, as calculated from the average of the assays previously made,

Gold	576
Silver	0·090

would have been only £965.

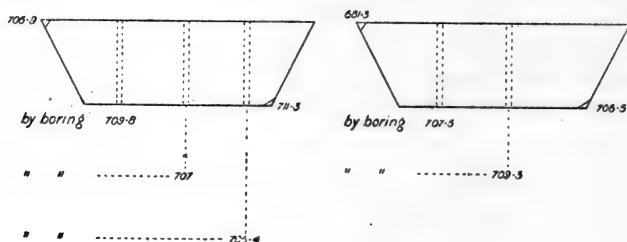
Analysis proved that the metals present other than gold were as follows :—

Silver	8.1
Lead	16.4
Zinc	9.5
Copper	4.0
Iron	0.3
Gold (by difference) ..	61.7

 100.0

The cause of the differences revealed by assays made from metal cut from various parts of the ingot was clearly due to liquation; but previous experience failed to afford any guide to the probable distribution of the precious and base metals in the ingot.

C. Another instance, and on a much larger quantity of gold alloy than the two former examples, was afforded by an ingot weighing 39.625 kilograms, which showed such great variation in its gold contents at various points that the ingot was re-melted and cast into two separate ingots, from which portions of metal were removed for assay by drilling.



All these results are the averages of assays made in *triplicate*, and a "dip" assay from the melted metal showed that it contained 701 parts of gold in 1000.

The analysis of this metal gave—

Zinc	7.1
Lead	4.9
Copper	4.8
Iron	1.4
Silver	9.2
Gold (by difference) ..	72.6

 100.0

As in the former case, the gold and silver present were isolated *in mass*, and the actual yield of fine gold and silver so obtained was as follows:—

Gold	27.914 kilograms.
Silver	3.568 „

which proved that the actual gold standard of the ingot was 703.9.

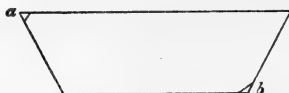
The base metal in two similar ingots was found by analysis to be composed as follows:—

	(492)	(494.)
Silver.....	8.9	8.0
Lead	9.0	7.7
Zinc	4.8	8.5
Copper	5.2	3.2
Iron	0.4	1.6
Nickel	0.8	1.8
Gold (by difference)..	70.9	69.2
	<hr/> 100.0	<hr/> 100.0

from which it would appear that the presence of one or both of the metals—zinc and lead—bears in some degree upon these variations in quality—it being well known that gold will alloy, and be constant in quality, with either silver or copper or with both in almost any proportions.

Advancing progressively, I now cite an instance of irregular distribution in a much baser alloy of gold.

An ingot of base gold alloy (P. 13) weighing 9.570 kilograms.



Determinations from the top of this ingot gave results:—

Point *a*—

Gold.	Silver.
265.0	—
378.4	213
383.0	—

From the bottom, point *b*—

527.2	—
560.0	66
545.5	—

From a “dip” taken from the fused alloy—

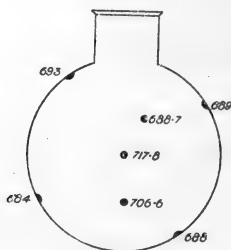
561.0	—
618.5	75
683.0	—

differences which are too significant to need comment.

In order to ascertain the effect exerted by these two metals—lead and zinc—in conjunction with gold, I prepared an alloy of 700 parts pure gold and 300 parts pure lead, and after mixing and casting into an open mould I cast the melted alloy into a spherical mould 2 in. in diameter, made of cast iron. Determinations made from different parts, after cutting the sphere into two halves, gave the following results, the assays being made in triplicate upon each portion of metal removed.

(The weight of this sphere was a little over 2 kilograms.)

FIG. 1.



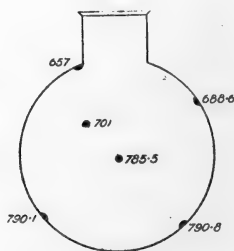
The result shows a decided tendency of the gold to *liquate to the centre of the mass*.

In the next experiment gold was alloyed with lead and zinc in the following proportions:—

Gold	75 parts.
Lead	15 „
Zinc	10 „

adding the zinc when the alloy of the first two metals was thoroughly fluid, and after casting this into an open mould. the alloy was remelted and cast into the 2-in. spherical mould before mentioned. This alloy was extremely hard and very brittle. Portions removed from the different parts of the sphere, after cutting it across, gave the following results:—

FIG. 2.



There is evidence of re-arrangement by liqutation in this case which sends gold to the centre, but the result is complicated, as gravity appears also to send gold to the lower portion of the spherical mass.

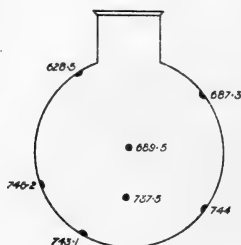
The foregoing mixture (No. 2) of

Gold	75 parts.
Lead	15 „
Zinc	10 „

was now further alloyed by the addition of 5 per cent. of pure copper and cast into a sphere which was very hard and brittle, and weighed about 2 kilograms.

The following are the results at the points shown :—

FIG. 3.



Here again, gravity appears to send gold to the lower portion of the sphere.

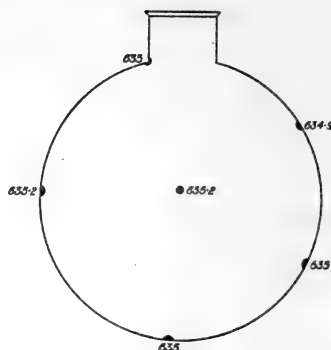
The question arises, does the silver play any part in the distribution of the baser metals, lead and zinc?

I therefore melted sphere No. 3 with 10 per cent. of silver, so that there were present :—

Gold	63.4 (by difference)
Silver.....	7.8
Copper	5.1
Zinc	8.8
Lead	14.5
Iron	0.4
	<hr/>
	100.0

and cast into an open mould, and subsequently into the spherical mould. The following were the results obtained of fine gold at the points indicated :—

FIG. 4.

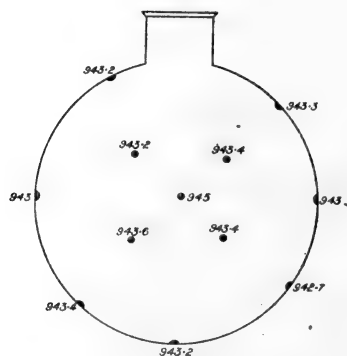


This sphere seems constant all over.

In order to see what was the effect with pure gold alloyed with metallic zinc only, I cast an alloy of fine gold with 5 per cent. of zinc into a 3-in. spherical mould. The weight of the sphere was 3.438 kilograms.

The results were as follows:—

FIG. 5.

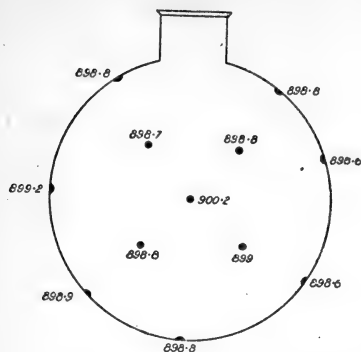


(Five per cent. zinc.)

A slight but decided tendency of liquation of gold towards the centre.

The same alloy, containing 95 per cent. of gold and 5 per cent. of zinc, was then alloyed with a further 5 per cent. of zinc and cast into the same sphere. This weighed 4.218 kilograms. The results were as follows:—

FIG. 6.

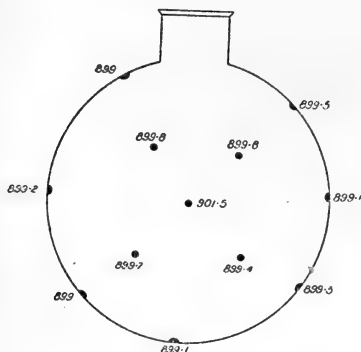


(Ten per cent. zinc.)

Feeling a little diffident about these results, I recast the foregoing alloy of gold with 10 per cent. of zinc, into the same mould.

The results were as follows :—

FIG. 7.

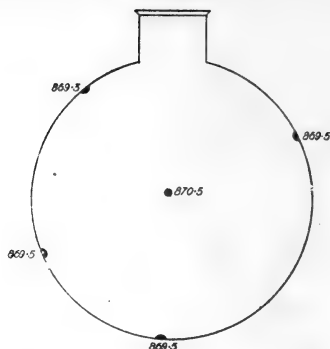


(Ten per cent. zinc.)

This shows that there is still a tendency in this gold alloy with 10 per cent. of zinc to become enriched towards the centre.

This 10 per cent. alloy was then alloyed with a further 5 per cent. of zinc and cast into the same spherical mould. The weight of this sphere was 4.021 kilograms. The results were :—

FIG. 8.



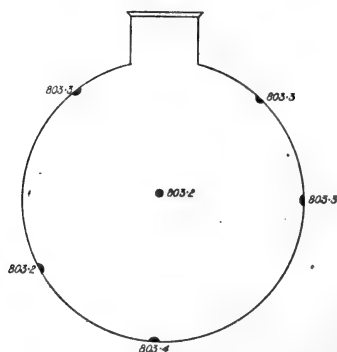
(Fifteen per cent. zinc.)

It is abundantly evident therefore, that zinc alone will not account for the differences in the ingots of impure gold; and the question arose, will the presence of a definite amount of silver in any way prevent the irregularity in composition?

To test this I alloyed the gold, which contained 15 per cent. of zinc so that it might also contain 7.5 per cent. of silver.

This was cast into the 3-in. sphere and weighed 3.934 kilograms, and assays made on portions of metal cut from it gave the following results :—

FIG. 9.



(Fifteen per cent. zinc.)

It was intended to contain—

Zinc	15.0
Silver	7.5
Gold	77.5
	<hr/>
	100.0

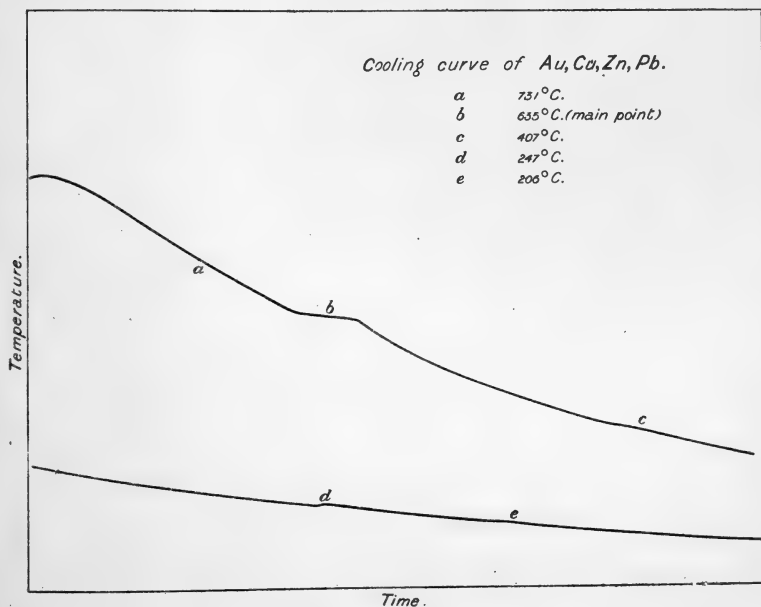
the extra richness of the gold over 77·5 being due to the volatilisation of the zinc. This experiment appears to confirm that on pp. 27, 28 (see results of fig. 4).

The foregoing experiments show that lead is far more effective as a cause of liquation than zinc, and the question arises, do zinc and lead separate into distinct layers by gravity when they are simultaneously present in a mass of gold, as they are known to do when they (lead and zinc) are melted together and allowed to solidify slowly. If they do separate, are they respectively associated with precious metal? Professor Roberts-Austen has given us a method of investigating such a problem. He has shown that it is easy to place a suitably protected thermo-junction in a mass of cooling alloy, and obtain by photography a record of the cooling of the mass,* a method which was employed by me for determining the temperatures at which the metals arsenic and antimony separate from bismuth. Applying this method to a mass weighing 44 grams of an alloy containing:—

Gold	75·0
Lead	15·0
Zinc	10·0

The following curve, No. I, is an autographic record of its solidification:—

CURVE NO. I.

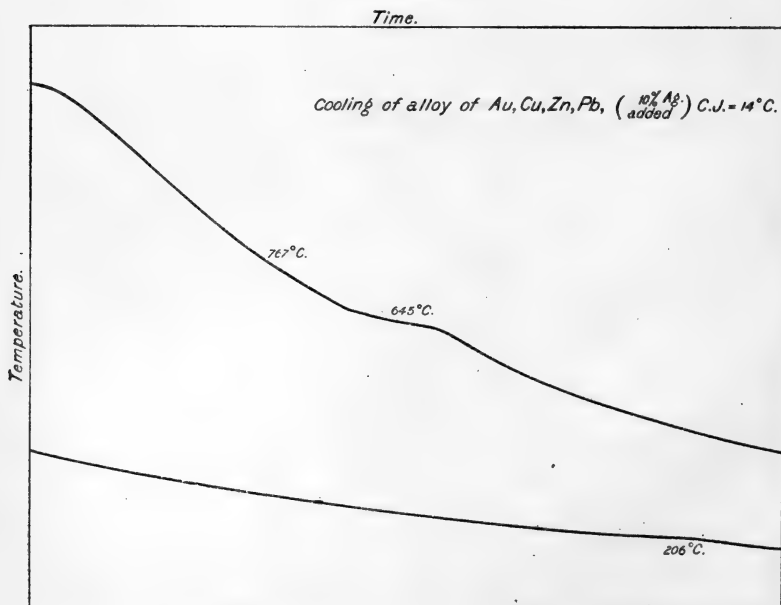


* See 'Roy. Soc. Proc.,' vol. 52, p. 467.

From this it will be evident, from the horizontal position (*b*) (of the curve No. I) that the mass solidifies as a whole at $635^{\circ}\text{C}.$; but there is a second break *c* in the curve at a temperature of $407^{\circ}\text{C}.$; and there is yet a third break at *d*, $247^{\circ}\text{C}.$ These latter points evidently are connected with the solidifying points of lead and zinc, but it is probable that these metals are, in solidifying, associated with some gold.

The second curve, No. II, represents the cooling of the same mass of gold with 10 per cent. of silver added. It will be seen that the metal has still one main solidifying point *b*, at $645^{\circ}\text{C}.$ The lower point (*c*) of the former curve is entirely absent, but there is an indication of the lead point at 206° . The results clearly indicate that silver is a solvent common to both zinc and lead, which are not, as in the previous case (Curve I) free to separate from each other. Such a mass should be fairly uniform in composition, and assays from different portions of it proved it to be so.

CURVE No. II.



The latter curve (II) seems to change its direction at 767° , which is above the main solidifying point of the mass, and it remains to be seen whether this is of any significance.

The inspection of the curves so obtained at once led me to infer that silver must be a solvent for zinc and lead when these are present

in gold, and with the clear indication thus afforded I proceeded to make the following experiments:—

The alloy—

Zinc	11.0
Silver	7.5
Gold	81.5
	<hr/>
	100.0

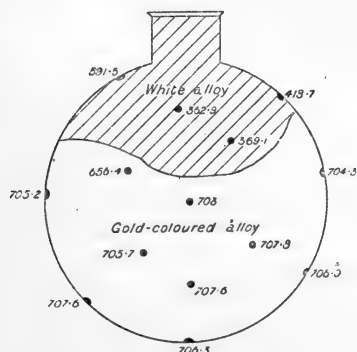
and weighing 5.680 kilograms, was now alloyed by the addition of lead to produce a similar metal to P. 13 (see p. 25), say:—

Zinc	10
Lead	20
Silver	7
Gold	63
	<hr/>
	100

and this was cast into two spheres, a 2-in. sphere and a 3-in. sphere.

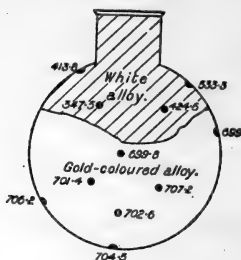
This alloy was so hard and brittle that I was compelled to cut these spheres into two by sawing them. When so cut asunder it was evident that the upper portions of both these spheres had a marked white appearance, as compared with the lower portions, which possessed the yellow colour of gold. The 3-in. sphere weighed 3.484 kilograms. Portions removed from these two spheres at the points indicated showed the following results:—

FIG. 10.



And those from the 2-in. sphere, weighing 0.880 kilogram —

FIG. 11.



Very marked separation takes place in both instances, the differences at various points of the sphere being very remarkable and forcibly illustrating the difficulties to which reference is made at the commencement of this paper.

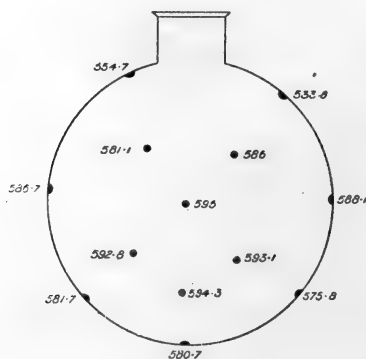
As, however, it appears, that when a certain amount of silver is present, the irregularity in composition disappears, I alloyed this mixture of—

Zinc.....	10
Lead.....	20
Silver	7
Gold.....	63

with more silver, so that it contained 15 per cent. of silver (nearly half the united amounts of zinc and lead present in the alloy).

This, cast into the 3-in. spherical mould, showed the following results at the points indicated. In *appearance*, the metal, when sawn in two, was homogeneous. The weight of the sphere was 3·459 kilograms.

FIG. 12.



There is still evidence of liquation of gold towards the centre, but comparison of fig. 12 with those which immediately precede it will show how greatly the arrangement of the alloy has been modified by the presence of the additional 8 per cent. of silver. The proportion of silver in this alloy was proved by assay to be 15.5 per cent.

As there was still evidence of liquation, the metal was cast with still more silver, making 20 per cent. of silver in all. The alloy, when cast into a mould, proved to be almost uniform in composition, the difference between the centre and the extreme portions being very slight.

Liquation had practically ceased, a fact which proves incontestably that silver is the solvent for the base metals, zinc and lead, when they are alloyed with gold.

Conclusions.—(1) Alloys of gold with base metals, notably with lead and zinc, now largely often met with in industry, have the gold concentrated towards the centre and lower portions, which renders it impossible to ascertain their true value with even an approximation to accuracy.

(2) When silver is also present these irregularities are greatly modified.

The method of obtaining "cooling-curves" of the alloys shows that the freezing points are very different when silver is present and when it is absent from the alloy.

(3) This fact naturally leads to the belief that if the base metal present does not exceed 30 per cent., silver will dissolve it and form a uniform alloy with gold.

(4) This conclusion is sustained by the experiments illustrated by figs. 9, 10, 11, 12, which, in fact, gradually lead up to it, and enable a question of much interest to be solved.

"On the Occurrence of the Element Gallium in the Clay-Ironstone of the Cleveland District of Yorkshire. Preliminary Notice." By W. N. HARTLEY, F.R.S., Professor of Chemistry, and HUGH RAMAGE, A.R.C.S.I., F.I.C., Assistant Chemist in the Royal College of Science, Dublin. Received April 13,—Read May 7, 1896.

In the course of an investigation of flame spectra at high temperatures ('Phil. Trans.,' A, vol. 185, pp. 1029—1091 (1894)) extended to the basic Bessemer process, the authors were occupied last July and August in observing the flames from the converters at the North Eastern Steel Company's Works, at Middlesbrough-on-Tees. A large number of photographs were taken in series during the progress of the "blow," and also of the "after blow," but these will

form the subject of another communication dealing with the chemistry of the process.

Some of the photographs were remarkably fine in definition, and they extended from the less refrangible limit of the red rays to the ultra-violet, about wave-length 3240.

It may be mentioned here, however, that every line and band in the different spectra was identified. Some of the photographs afforded evidence of very unusual constituents in the mixture of gases and vapours, which by their combustion and incandescence give the Bessemer flame. The identity of these could have been established only by means of very complete investigation of oxy-hydrogen blowpipe spectra. Apart from all technical considerations which were kept in view, and of such purely scientific questions as were involved in similar previous researches carried out by one of us, the examination of these spectra was of great interest, more especially because of the proof of the rare element, gallium, being present in the Bessemer metal, and in the roasted ore from which it was extracted. It was shown by very careful analyses that the gallium was concentrated in the iron, but all details of the operations involved in its separation and of the quantitative determinations are reserved for a future communication.

The evidence of the existence of gallium in the ore and in the metal rests on the measurements of the wave-lengths of the lines in a large number of photographed spectra and upon the relative strengths of the lines in the different materials examined and in the precipitates obtained therefrom.

The following examples show the nature of this evidence:—

1. *Evidence from the Bessemer Flame Spectra.*

Seventy-six of the photographed spectra of the Bessemer flame contain a strong line with wave-length about 4171·5, which does not appear to be related to any other line in these spectra, and belongs, therefore, to some other element than those otherwise identified.

2. *Evidence from the Spectrum of the "Mixer Metal" and of the different substances separated by its Chemical Treatment.*

The "mixer metal" heated in the oxy-hydrogen flame gives a spectrum of iron with a strong line having a wave-length of 4171·6.

The residue left after dissolving the iron by boiling with hydrochloric acid also gives this line 4171·6 very strongly.

Precipitates obtained by boiling the solution of the iron with ammonium acetate give the line 4171·6 and also a weaker line, wave-length 4032·7.

The latter line is seen only in the absence of manganese, as it very nearly coincides with one of the group of strong manganese lines; it is, therefore, obscured in the spectra of the Bessemer flame and of the crude iron.

The oxide of gallium was separated as far as possible from all other substances and heated in the oxy-hydrogen flame and the characteristic spectrum was then photographed from this oxide.

3. *Evidence from the Roasted Ore, and substances separated therefrom.*

The roasted Cleveland ore was heated alone for thirty-five minutes in the oxy-hydrogen flame, it gave only a very faint indication of one line in the spectrum of gallium. The solution extracted from the ore by digesting it with warm dilute hydrochloric acid of double normal strength, when boiled with ammonium acetate gave a precipitate, the spectrum of which contained the line 4171.6 *fairly strong*.

The silicious residue insoluble in *strong* hydrochloric acid, when decomposed by fusion with caustic potash and subsequent boiling with water, after concentration of the solution so as to retain the gallium, gave a spectrum containing both lines, 4171.6 and 4032.7. All other elements had been removed.

The wave-lengths given are on Rowland's scale. The lines were measured on many plates and also repeatedly on the same plate, the results being the same in each case.

“The Electromotive Properties of the Electrical Organ of *Malapterurus electricus*.” By FRANCIS GOTCH, M.A. (Oxon.), F.R.S., and G. J. BURCH, M.A. (Oxon.). Received April 2,—Read May 7, 1896.

(Abstract.)

The experiments were made upon six specimens of *Malapterurus electricus*, 12 to 15 cm. in length, brought from the River Senegal by Mr. A. Ridyard (ss. “Niger”), and generously placed at the disposal of the authors by the Liverpool Corporation Museum Committee, to whom and to Dr. Forbes, the Director of the Museum, the authors desire to express their thanks.

Three of the specimens were killed, in order to carry out experiments upon the isolated organ. The instrumental methods employed by the authors for determining for the first time the characters and time relations of the activity of the organ response were chiefly the following:—

- (a.) The record of the frog nerve muscle galvanoscope.
- (b.) The galvanometer connected with a suitable rheotome.

(c.) The capillary electrometer, a large number (about 250) photographic records being taken of the movements of the meniscus. Facsimile reproductions of typical records are given in the fuller communication. The electrometer was used either shunted by a resistance of from 80 to 100 ohms, or in connection with the outer plates of a special condenser, the inner plates of which were connected with the fish or its electrical organ.

The organ responded to mechanical or electrical excitation of its nerves after removal from the fish, the response being unaffected by 1 per cent. curare, or 1 per cent. atropine solution. No response could be evoked by such chemical agents as sodium chloride, glycerine, or weak acid, when applied either to the organ or its efferent nerve.

The conclusions drawn by the authors from the experiments on the isolated organ and on the entire uninjured fish may be summarised as follows :—

(1) The isolated organ responds to electrical excitation of its nerves by monophasic electromotive changes, indicated by electrical currents which traverse the tissue from the head to the tail end; this response commences from 0.0035" at 30° C. to 0.009" at 5° C. after excitation, the period of delay for any given temperature being tolerably constant.

(2) The response occasionally consists of a single such monophasic electromotive change (shock) developed with great suddenness, and subsiding completely in from 0.002" to 0.005", according to the temperature; in the vast majority of cases the response is multiple, and consists of a series of such changes (shocks) recurring at perfectly regular intervals, from two to thirty times (peripheral organ rhythm); the interval between the successive changes varies from 0.004" at 30° C. to 0.01" at 5° C., but is perfectly uniform at any given temperature throughout the series.

(3) Such a single or multiple response (in the great majority of cases the latter) can also be evoked by the direct passage of an induced current through the organ and its contained nerves, in either direction heterodromous (*i.e.*, opposite in direction to the current of the response) or homodromous.

(4) The time relations of the response are almost identical whether this is evoked by nerve-trunk (indirect) stimulation, or by the passage of the heterodromous induced current.

(5) There is no evidence that the electrical plate substance can be excited by the induced current apart from its nerves, *i.e.*, it does not possess independent excitability.

(6) The organ and its contained nerves respond far more easily to the heterodromous than to the homodromous induced current, and the period of delay in the case of the latter response is appreciably lengthened.

(7) The peripheral organ rhythm (multiple response) varies from about 100 per second at 5° C. to about 280 per second at 35° C.

(8) One causative factor in the production of the peripheral rhythm is the susceptibility of the excitable tissue to respond to the current set up by its own activity (self excitation).

(9) In the uninjured fish mechanical or electrical excitation of the surface of the skin beyond the limits of the organ evokes a reflex response with a long delay (0.03" to 0.3"); this reflex response consists of groups of shocks, each group showing the peripheral organ rhythm, but separated from its neighbour by a considerable interval of time (reflex or central rhythm).

(10) In the uninjured fish electrical excitation of the skin over the organ evokes a response which may consist of a direct peripheral organ effect followed by such a reflex effect.

(11) The minimal total reflex delay at 20° C. is 0.023", giving a central excitatory time of about 0.01".

(12) The reflex or central rhythm in our specimens showed a maximum rate of 12 per second and an average rate of from 3 to 4 per second.

(13) The number of separate groups in the reflex response recurring at the intervals mentioned in the preceding paragraph was in our fish limited to from 2 to 5.

(14) The E.M.F. of each single change in the organ response depends upon the number of effective plates with their nerves, and in 10 cm. of excited organ cannot possibly be less than 75 volts, and is probably much nearer 150 volts. As in our specimens the number of plates in series in 1 cm. of organ was 180, this gives a minimal possible E.M.F. of 0.04 volt, and a probable E.M.F. of 0.07 volt for each plate.

The authors further conclude that, since each lateral half of the organ is innervated by the axis cylinder branches of one efferent nerve cell, and has no independent excitability, the specific characters of the reflex response of the organ express far more closely than those of muscle the changes in central nerve activity, and are presumably those of the activity of a single efferent nerve cell.

The single efferent nerve cell, the activity of which is thus for the first time ascertained, shows—

(a.) A minimum period of delay of 0.008" to 0.01".

(b.) A maximum rate of discharge of 12 per second.

(c.) An average rate of discharge of 3 to 4 per second.

(d.) A susceptibility to fatigue showing itself in the discharge failing after it had recurred from two to five times at the above rates.

“The Occurrence of nutritive Fat in the Human Placenta. A Preliminary Communication.” By THOMAS WATTS EDEN, M.D., M.R.C.P. Communicated by Dr. PYE SMITH, F.R.S. Received April 23,—Read May 7, 1896.

(From the Laboratories of the Conjoint Board of the Royal Colleges of Physicians (Lond.) and Surgeons (Eng.)).

Recently, while examining specimens of ripe placenta for fatty degeneration, I was struck by the regularity of the occurrence of fat in this structure, and especially by the nature and extent of its distribution. I was then led to examine a series of specimens taken at different periods of gestation, with the result that a free deposit of fat was found in ten different placenta, all of which I believe to be non-pathological, and ranging practically through all periods of gestation, from the sixth week up to term.

The method employed for the demonstration of this fat, was to take slices from different parts of the placenta, and harden them for a few days in Müller's fluid; then to transfer thin strips, not exceeding one-third of an inch in thickness, to Marchi's fluid (1 per cent. solution of osmic acid 1 part, Müller's fluid 2 parts) for a week. The pieces were then embedded in paraffin, cut with a rocking microtome, and stained lightly with saffranine, eosine, or logwood and eosine, or mounted unstained. By this process the fat is completely blackened, while the other tissues retain their normal staining reactions, so that the outlines of the fat-containing cells can be distinctly made out.

By this method I have been able to demonstrate the constant occurrence of fat in certain well-defined regions of the human placenta.

In the young human placenta, the epithelial covering of the villi consists of two layers, a superficial, nucleated, plasmodial layer, and a deep cellular layer. In a six weeks' ovum I found fat in the form of minute droplets in both these layers, but much more abundantly in the former than in the latter. These fat droplets show comparatively little variation in size, and they remain discrete, showing little or no tendency to form larger droplets by fusion; they are confined to the perinuclear protoplasm, and are never found in the nuclei, which remain unaltered in number, form, and arrangement. The stroma of these villi contains here and there a trace of fat, but it is apparently healthy, and is furnished with well-formed wide capillaries filled with blood. The villi are, in fact, to all appearance healthy. Every villus does not show this deposit of fat, but it is present in very large numbers of them; in every field of the microscope several villi

containing fat may be found. The amount of fat also varies considerably.

In a young ovum the plasmodial layer of the villi shows great proliferative activity; it throws out numerous club-shaped processes or buds, which represent the first stage in the development of new villi. These buds very frequently contain large numbers of minute fat droplets. I believe that this is a point of very great importance, showing, as it does, that the deposit of fat occurs in actively growing tissues of undoubted vitality.

In the ripe placenta the proliferation of the plasmodial layer has ceased, and degenerative changes are present in scattered regions. But, of course, the great majority of the villi retain their vitality, and in these villi a free deposit of fat is present, showing the same distribution and characters as in the young placenta.

I have also found a similar deposit of fat in the serotina. The six weeks' ovum, above referred to, showed very many decidual cells containing minute, discrete droplets of fat in the perinuclear protoplasm. A placenta of the sixth month also showed an abundant fat deposit in the same region. At term, the serotina shows many degenerative changes, and although it contains fat, it may well be doubted whether, at this period, this is a physiological deposit.

The placenta, indeed, appears to be a storehouse of nutritive fat, just as is the liver. This appears to throw some light on what has long been one of the problems of foetal physiology, viz., the source from which the foetus obtains its supplies of fat. Diffusible substances such as sugar, salts, peptones, &c., were supposed to pass by osmosis from the maternal blood in the inter-villous spaces, to the foetal blood in the villi. But this could not be assumed of indiffusible substances such as fat. The truth would seem to be that fat is deposited from the maternal blood in the epithelium of the villi, and stored up there by the foetal tissues for their use. No great accumulation of fat occurs, as it appears to be from time to time absorbed and disposed of by the foetal circulation. It is, however, not altogether clear how a deposit of fat in the decidual cells can be made available for the purposes of foetal nutrition.

Since finding this fat deposit in the human placenta, I have begun a series of comparative observations upon the placenta of other mammals. Up to the time of writing, I have examined two rabbits' placenta, one from an early, and the other from a late, period of gestation. In both there was a marked deposit of fat, chiefly in the superficial glandular layer of the maternal placenta, but also, though to a less extent, in the processes of the chorionic mesoblast, which form the homologues of the villi of the human placenta.

The process appears to correspond closely to that observed by Mr. George Brook, in the transmission of fat from the yolk to

the segmenting germinal area, by the parablast of mesoblastic ova.*

I was under the impression when these observations were made, that fat had never been found, in this form, in the placenta before. I find that I am to some extent anticipated by a paper in the 'Archiv für Gynaekologie,' February, 1896.† One of the authors (Aschoff) wished to examine a malignant uterine growth, which he believed to be of the nature of *Deciduoma malignum*. Before doing so, he examined several specimens of young human ova, in order, as he says, to learn something of the structure of growing chorionic villi. Some of the specimens he hardened in Flemming's solution, and in all of these he found fat in the plasmodial layer of the villi. Aschoff's description of the fat deposit agrees very closely with that already given of my own specimen. "An den Flemmingschen Präparaten ist das Syncytium dadurch ausgezeichnet, dass es in seiner Randzone eine dichte Anhäufung feinsten Fetttröpfchen trägt. Dieselben sind bald sehr fein, bald grobkörnig, aber in den betreffenden Abschnitten des Syncytiums stets von gleicher Grösse Die Fetttröpfchen überall sich finden, wo Chorionepithelzellen, in directesten Stoffwechselaustausch mit den Intervillösenräumen treten" (p. 531).

Aschoff scarcely appreciates the physiological importance of the observation, but there can be no doubt that his observations and my own are mutually confirmatory.

"Note on the Larva and the Postlarval Development of *Leucosolenia variabilis*, H. sp., with Remarks on the Development of other Asconidæ." By E. A. MINCHIN, M.A., Fellow of Merton College, Oxford. Communicated by Professor E. RAY LANKESTER, F.R.S. Received April 25,—Read May 21, 1896.

Introductory Remarks.

Through the kind hospitality of Professor de Lacaze-Duthiers, I was able to spend the spring and summer of last year at the marine laboratories of Banyuls-sur-Mer and Roscoff, where I was chiefly engaged in studying the embryology of the Ascons. In Banyuls I obtained the larvæ of *Leucosolenia cerebrum*, H. sp., in June, and of *L. reticulum*, O.S. sp., in July. In Roscoff I found the larvæ of *L. variabilis*, H. sp., all through August and the early part of September.

* "Formation of the Germinal Layers in Teleostei," 'Roy. Soc. Edin. Trans.,' 1896.

† "Ueber bösartige Tumoren der Chorionzotten," Apfelstedt und Aschoff.

and of *L. coriacea*, Mont. sp., in September. Owing to the inexperience with which I approached the difficult task of rearing these larvæ, my results are not so complete in all details as I could wish, but in the case of *L. variabilis* I was able to obtain a more or less perfect developmental series, and in the other three species I was able to make out satisfactorily the main points in the metamorphosis, especially the important question of the relation between the cell-layers of the larva and those of the adult. I hope to bring my investigations to completion during the present year, but, in the meantime, the results obtained seemed to me of sufficient importance to form the subject of a preliminary note. The material which I collected and preserved was further studied at Munich, in the laboratory of Professor Richard Hertwig, to whom I am indebted for much kind help and advice, as well as hospitality.

The Development of Leucosolenia variabilis (Ascandra variabilis, H.).

The larvæ of *L. variabilis* are of the so-called amphiblastula type, but in many respects more primitive than the amphiblastula larva hitherto described in other Calcarea. The minute larvæ (70—80 μ in length, 50—60 μ in breadth) leave the mother sponge by the osculum, and at once rise to the surface of the water, where they swim for about twenty-four hours. They then sink to the bottom, where, after swimming about slowly for twelve to twenty-four hours more, they fix themselves and undergo metamorphosis. The larval life thus lasts for thirty-six to forty-eight hours.

The oval larva (figs. 1 and 2)* is divided into an anterior region composed of ciliated cells and a posterior region composed of non-ciliated granular cells. The centre of the transparent larva is occupied by a conspicuous mass of yellowish-brown pigment. The ciliated cells are slender and elongated, reaching from the pigment to the surface of the body. Each cell bears a single flagellum, and the body of the cell is divided into an internal refractile portion and an external granular portion. These two portions of the cell are so distinct in the living object that a superficial examination gives the impression of an internal layer of refractile cells covered by an external granular layer, but by more careful investigation it is easy to make out that these two apparent layers are merely parts of a single layer of cells. The ciliated cells situated more posteriorly entirely lack the refractile inner portion, and appear granular throughout. They are also slightly broader, and have more convex outer surfaces than the other ciliated cells, forming an equatorial zone of *intermediate cells*, not very distinct in the living object. The

* Figs. 1—6 represent the development of *L. variabilis*, $\times 1000$ diameters. All but 1 and 2 are semidiagrammatic and combined from different preparations.

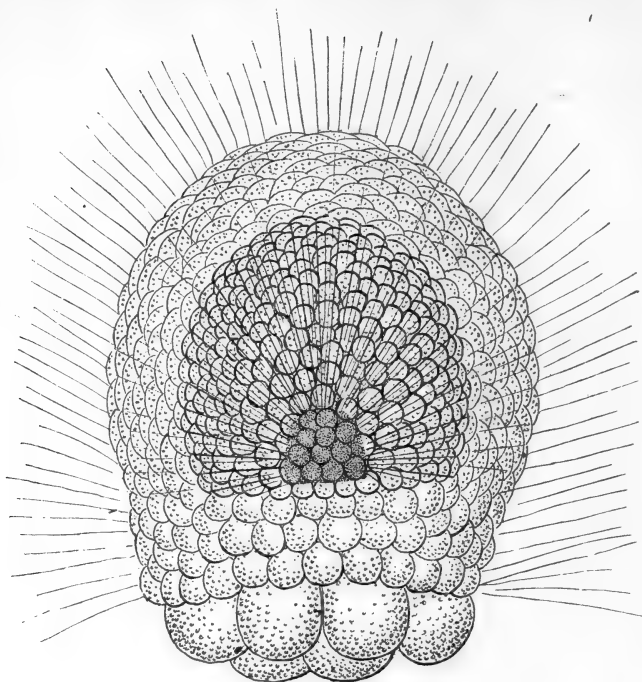


FIG. 1.—Newly hatched larva.

region of the intermediate cells is generally marked by a slight constriction, giving a waist, as it were, to the larvæ. The granular cells are much fewer in number than the other elements, and are also of much larger size, but there are gradations in this respect, those placed at the posterior pole being much larger than those which border upon the intermediate cells.

During the free-swimming larval period, considerable changes take place in the relative proportions of the different parts of the larvæ. In the newly hatched larva (fig. 1) the anterior ciliated region is relatively large, with a very broad granular border to the cells, and the posterior granular cells are few in number. The number of granular cells now increases at the expense of the ciliated cells. Some of the ciliated cells, by absorption of the internal refractile portion of the cell, become intermediate cells, and these, in their turn, absorb their flagellum, increase in size, and become granular cells. This process goes on *pari passu* with a decrease in the granular border of the ciliated cells. In the larva of about twenty-four hours (fig. 2), the granular cells form a mass equal to that of the ciliated cells, and the latter have now a very narrow granular border. In

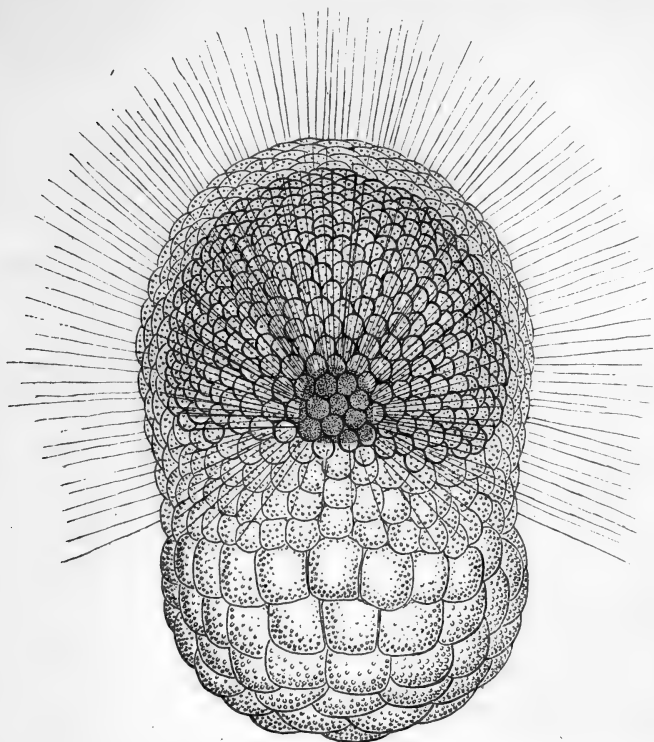


FIG. 2.—Larva of second day.

short, granular cells are formed during larval life by modification of ciliated cells, the intermediate cells being a stage in this process.

Sections of larvæ confirm and amplify the results obtained from a study of the living object (fig. 3). The inner portion of each ciliated cell, which in life appeared refractile, is seen to contain a series of vacuole-like structures, containing granular masses suspended in their interior. At the junction between the internal vacuolated and external granular portions of the cell is situated the opaque and deeply staining nucleus, which has a form like an onion, and is continued externally into the flagellum. Often the inner side of the nucleus is indented by the vacuole beneath it, sometimes to such an extent that the nucleus has the form of a crescent in section. The intermediate cells are very distinct in sections, and by some methods of preservation and staining, *e.g.*, osmic acid followed by picrocarmine, their protoplasm takes up the stain in a remarkable manner, so that larvæ treated in this way appear to have a brightly coloured equatorial zone. They lack the vacuolated inner portion, characteristic of the

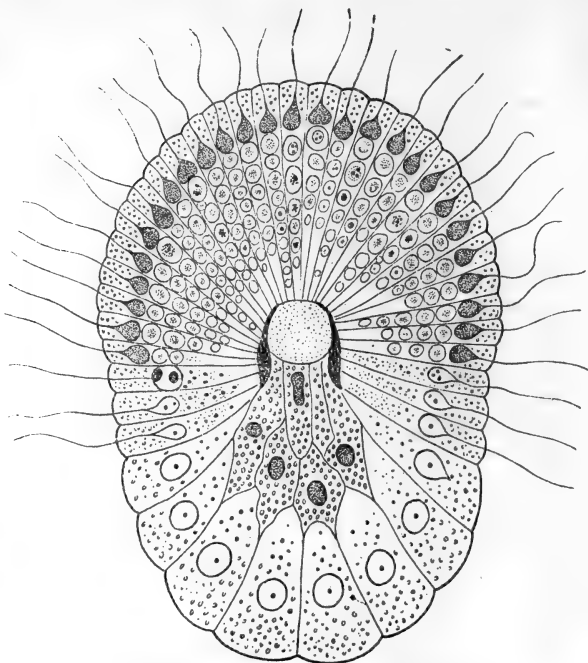


FIG. 3.—Longitudinal section of larva.

ciliated cells proper, and their nuclei are larger and paler with one or two nucleoli. The nucleus of the first intermediate cell frequently presents a curious appearance, being swollen out into a large vesicular structure containing two or three chromatin masses. This condition is apparently in connexion both with a process of rearrangement of the chromatin and with the absorption of the vacuoles. The granular cells are arranged in a single layer, and have large pale nuclei with nucleoli. Often the nucleus of the cell nearest the intermediate cells has a pointed outer end, evidently indicating the former connexion with the flagellum.

Sections reveal a remarkable set of structures in connexion with the central pigment, which is now seen to have the form of a tube, open in front and behind, and enclosing a rounded, lens-like body, apparently a gelatinous mass filling the central cavity, the remnant, doubtless, of the segmentation cavity. Behind these bodies are a number of cells with coarse granules and small, very opaque, deeply staining nuclei.* One of these cells is placed in the longitudinal axis

* Cf. Dendy's account of the larva of *Grantia labyrinthica* for similar cells, "On the Pseudogastrula stage in the Development of Calcareous Sponges," 'Roy. Soc. Victoria Proc.,' 1889, pp. 93—101.

of the larva, and its nucleus is usually, but not always, elongated in the same direction, so as to have a rod-like form. The whole structure, with pigment, lens-like body, and central granular cells, gives strongly the impression of a primitive, light-perceiving organ. The pigment itself is lodged in the inner ends of the ciliated and intermediate cells, and is, no doubt, the same pigment as that observed by Metschnikoff* and Schulze† in the inner ends of the ciliated cells in the larva of *Sycandra raphanus*. As the intermediate cells pass into the condition of granular cells, they leave the pigment behind, so that the pigment is thickest in the region of the intermediate cells, at the sides of the lens-like body.

The larva is thus composed of four kinds of cells, which may be termed the ciliated, intermediate, granular, and central cells. Since the intermediate cells are merely a transitional form between the ciliated cells proper and the granular cells, we have to reckon with three classes of cells only in the fully developed larva.

The fixation takes place by the anterior pole of the larva, and the granular cells grow round the ciliated cells. The metamorphosis is complete in a few hours. Sections of fixed stages of the first day of fixation (fig. 4) show them to be composed of two very distinct cell

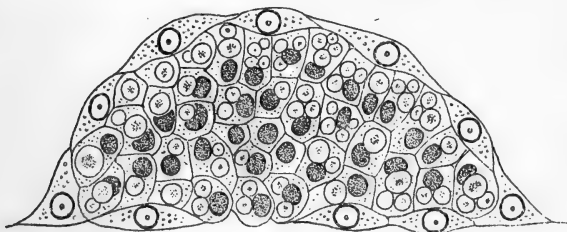


FIG. 4.—Section of larva shortly after fixation, the metamorphosis not quite complete.

layers: (1) a compact central mass of cells, easily recognisable, by their opaque, irregularly shaped nuclei and vacuolated cell protoplasm, as the former ciliated cells, surrounded by (2) a single layer of flattened epithelial cells, the former granular cells of the larva. No trace is to be found of the central cells, which appear to be thrown out, together with the pigment, at the metamorphosis. The inner mass is the future *gastral* layer of the sponge, the outer epithelium the future *dermal* layer.

* "Zur Entwicklungsgeschichte der Kalkschwämme," 'Zeitschr. f. Wiss. Zool,' vol. 24, pp. 1—14, Taf. I.

† "Ueber den Bau und Entwicklung von *Sycandra raphanus*," *ib.*, vol. 25, suppl., pp. 247—280, Taf. XVIII—XXI.

The two component layers very soon begin to undergo changes of form and structure, which are best described separately, since the two layers develop more or less independently of one another, and a given stage in the development of one layer is not always found combined with one and the same stage in the development of the other.

The dermal layer becomes divided (fig. 5) into two kinds of cells: (a) cells which retain the original form and characters and remain on the surface, and (b) cells with smaller nuclei, which sink below the outer epithelium and form a scattered layer between it and the

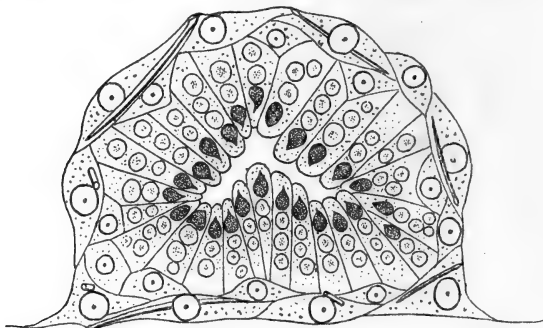


FIG. 5.—Section of stage about twenty-four hours after fixation. The left side is represented as slightly in advance of the right side.

gastral cells. The former (a) secrete each a single monaxon spicule, which appears first on the inner side of the nucleus, but soon grows out and projects free from the surface. The latter (b) unite into groups and secrete the triradiate spicules. The monaxons appear first, as in *Sycandra raphanus*,* and begin to appear about twenty-four hours after fixation, the triradiates about twelve hours later. The dermal layer has thus become divided into two parts, which gradually assume the adult characters. I have not observed the origin of the pores.

The gastral layer, at first a compact mass with no definite arrangement, soon begins to form a cavity (fig. 5). The cells assume a radiate arrangement, and a split-like lumen appears in the centre. Sometimes two or more such lacunar spaces arise, at first quite independent of one another, but later fusing to form a single gastral cavity, which soon becomes very large, causing the larva to increase considerably in size as a whole. At first the cavity is surrounded on all sides by gastral cells, but as it increases in size a spot appears where gastral cells are wanting, and the cavity is limited only by dermal cells (fig. 6). This is the region of the future osculum, and the dermal cells at this spot form the future oscular rim, where collar

* Metschnikoff, *loc. cit.*

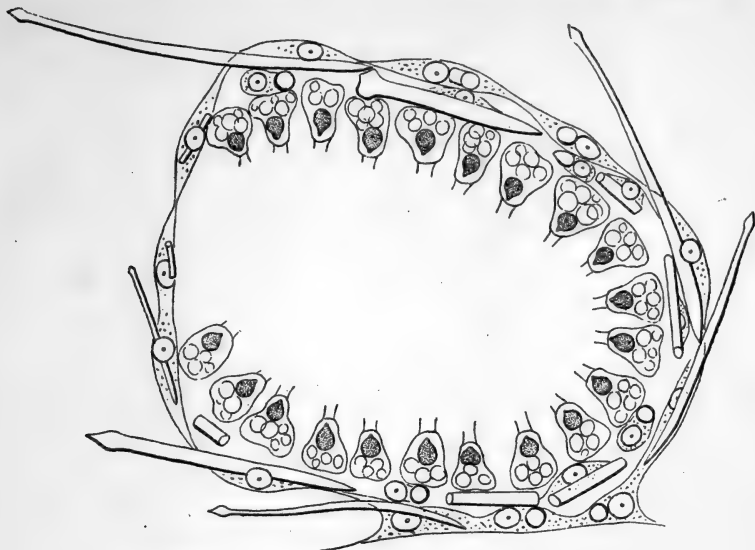


FIG. 6.—Section of stage of about the fourth day of fixation.

cells are lacking. The gastral cells are at first elongated, but later become shorter, and take on the characteristic appearance of collar cells. I have not been able to make out whether all the gastral cells become collar cells, or whether some of them do not become the wandering cells of the adult, which seems very probable. The osculum appears about the sixth day of fixation.

The Development of Leucosolenia cerebrum, H, *L. reticulum*, O. S.,
and *L. coriacea*, Mont.

These three species have larvæ of the type with which we are familiar from the descriptions of Metschnikoff* and Schmidt,† namely, oval ciliated blastulæ, in which an inner mass is formed by immigration of cells into the interior. The process is most easily followed in the more transparent larva of *L. reticulum* (fig. 7), where the modification of ciliated cells into granular cells, and their subsequent immigration, takes place at the posterior pole. When the larva is ready for fixation, a considerable quantity of granular cells has been formed, though the cavity is far from being obliterated. In the opaque larvæ of *L. cerebrum* and *coriacea* the process is more

* "Spongiologische Studien," 'Zeitschr. f. Wiss. Zool.,' vol. 32, p. 362, Taf. XXIII.

† "Das Larvenstadium von *Ascetta clathrus* und *Ascetta primordialis*," 'Arch. f. Mikr. Anat.,' vol. 14, pp. 249—263, Taf. XV, XVI.

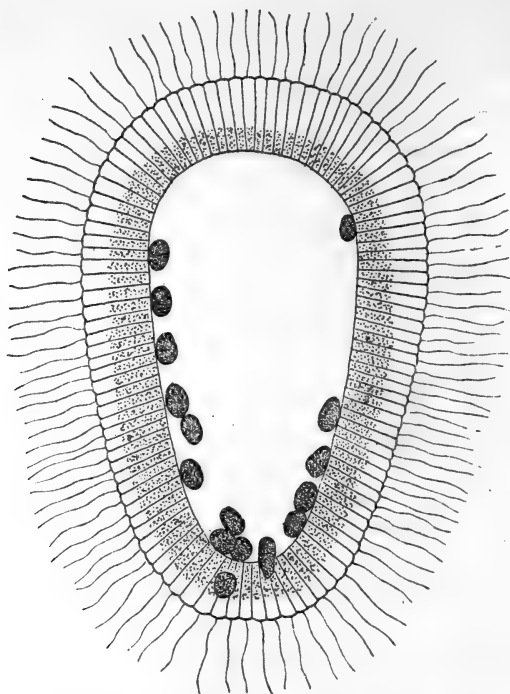


FIG. 7.—Optical section of larva of *L. reticulum*, first day, $\times 500$.

difficult to follow, but in both immigration appears to take place from any point on the surface.

In *L. cerebrum* and *L. reticulum* the larva swims for about twenty-four hours at the surface, and as long at the bottom, and fixes on the third day. *L. coriacea*, on the other hand, is remarkable for its abbreviated larval period as compared with the two Mediterranean species, since the larva fixes in a few hours, a fact doubtless in connexion with its life between tide marks, where the violent currents to which it is exposed renders a very sheltered, and therefore limited, habitat necessary for so delicate an organism.

After fixation, the larva undergoes changes whereby the ciliated cells become surrounded by the formerly internal granular cells, so that the ciliated external layer of the larva represents the gastral layer of the adult, while the inner mass becomes the dermal layer; the reverse of what was supposed by Metschnikoff and Schmidt (*loc. cit.*) to take place.

In *L. cerebrum* I was able to observe the first appearance of the spicules. As in *variabilis*, the complete metamorphosis results in a stage in which the gastral cells form a compact internal mass, sur-

rounded by a single layer of dermal cells. Some of the cells of the dermal epithelium then form themselves into groups, usually of three cells, and each cell of such a group secretes the ray of a spicule. The first spicules are usually triradiate, but quite irregular in form, and at their first appearance they are quite superficial, their secreting cells forming part of the general epithelium, but later they become covered by the remaining epithelium, so that the dermal layer becomes divided into an internal connective tissue layer and an external flat epithelium. The process is essentially similar to that occurring in *variabilis*, except that in the latter the cells of the flat epithelium secrete each a monaxon spicule, which in *cerebrum* is not the case.

General Considerations.

The larva of *L. variabilis* is of interest as affording a transition from larvæ such as that of *L. reticulum*, to the amphiblastula larva of the Sycons. The larva of *reticulum* (fig. 7) is composed of (1) ciliated cells, comparable to those of the amphiblastula, of which some (2) at the hinder pole are undergoing modification, and may be compared with the intermediate cells, and of (3) internal granular cells comparable to the posterior granular cells of the amphiblastula. To obtain a larva like that of *variabilis* from the type represented by *reticulum*, we must suppose the large cavity of the latter reduced to the extent to which this has occurred in the former. Then the granular cells which are formed at the posterior pole must remain where they are, since the cavity is too small to contain them, and, as more ciliated cells are continually being modified around them, we get a larva with the three kinds of cells arranged as in *variabilis*. The central cells of *variabilis*—on the origin of which I have no observations to bring forward—are probably to be regarded as constituting a larval organ, a special adaptation of no importance for the postlarval development.

The development of both *reticulum* and *variabilis* points to an early stage in which the larva is composed entirely of similar and equivalent ciliated cells. I have not seen such a stage in any species, and doubt if it actually occurs in nature; it is more probable that the process of cell differentiation begins before the larva is hatched in all cases. In the absence of segmentation stages, it is impossible to decide this question; nevertheless, the facts seem to me to indicate, as the primitive larva in ascon phylogeny, a blastula composed of indifferent ciliated cells, in which a second type of cells (the future dermal layer) is formed by modification of certain of the cells. The collar-cell layer of the adult is derived directly from the primitive ciliated cells of the blastula.

Comparing, now, the larva of *variabilis* with that of *Sycon raphanus*,

as described by Schulze, it is obvious that the development is essentially similar in both, the chief difference being with regard to the periods at which the various events take place. In both the granular cells increase greatly in number, but in *raphanus* this takes place while the larva is still in the maternal tissues, as is obvious from Schulze's figures,* and the larva is hatched in a condition similar to that of *variabilis* when about to fix. In *variabilis* the granular cells do not surround the ciliated cells until after fixation; in *raphanus* this process is begun while the larva is still swimming, and the granular cells may even give rise to spicules (monaxons) during the free swimming period (Metschnikoff, *loc. cit.*). It is obvious that in *Sycon* we have before us a hastening and shortening of the development, and, allowing for these embryological adaptations, we are able to understand how, from a larva such as that of *reticulum*, there has arisen a type of development apparently so different as that of the *Sycon* amphiblastula.

The most important event in the post-larval development is the differentiation of the dermal layer into the outer epithelium and the inner connective tissue layer. This might seem at first sight to be a process comparable to the formation of a new layer, a mesoderm; so that from this period onwards the sponge would be a three-layered organism. I do not, however, take this view, for the following reason. The immigration of cells from the epithelium to form the layer of triradiates is not an event, like the formation of a germ layer, which takes place once and for all in the life cycle of an individual, but it goes on whenever new triradiates are formed. In adult ascons I have found that the triradiates and the basal rays of the quadriradiates arise from cells of the outer epithelium which migrate inwards and arrange themselves into groups to form spicules, each ray being secreted by one cell or by cells derived from the division of a single cell. In the adult also the nuclei of the spicule secreting cells diminish in size after quitting the epithelium. Hence in the development of the sponge also, I regard this process as one not of blastogenetic, but of histogenetic significance. The fact that in *variabilis* the epithelial cells also secrete spicules is to my mind a decisive proof of the unity of the dermal layer.†

* 'Zeitschr. f. Wiss. Zool.,' vol. 25, suppl., Taf. XX and fig. 3, Taf. XIX. Schulze refers this increase in the number of the granular cells to their multiplication by cell-division, but as the granular cells do not at the same time decrease in size, it seems more probable that their increase is due, as in *variabilis*, to their numbers being recruited from the clear (ciliated) cells.

† Schulze has also figured very clearly the relation of the dermal cells to the monaxon spicules, one spicule to each cell, in the young fixed stages of *Sycon raphanus* ('Zeitschr. f. Wiss. Zool.,' vol. 31, pl. XIX, figs. 10, 11), although he states in the text that the spicules arise in the hyaline substance between the two layers.

“Helium and Argon. Part III. Experiments which show the Inactivity of these Elements.” By WILLIAM RAMSAY, Ph.D., F.R.S., and J. NORMAN COLLIE, Ph.D., F.R.S.E.
Received April 22,—Read May 21, 1896.

To chronicle a list of failures is not an agreeable task; and yet it is sometimes necessary, in order that the record of the behaviour of newly discovered substances may be a complete one. It is with this object that we place on record an account of a number of experiments made to test the possibility of forming compounds of helium and argon.

It will be remembered that in our memoir on Argon,* Lord Rayleigh and Professor Ramsay described numerous experiments, made in the hope of inducing argon to combine, all of which yielded negative results. Two further experiments have been since made—again without success.

1. The electric arc was maintained for several hours in an atmosphere of argon. The electrodes were thin pencils of gas carbon, and, previous to the introduction of the argon, the arc was made in a vacuum, and all gas evolved was removed by pumping. Argon was then admitted up to a known pressure, and the arc was again made. A slow expansion took place; one of the electrodes diminished in length, and the bulb became coated with a black deposit. The resulting gas was treated with caustic soda and with a solution of ammoniacal cuprous chloride, and, on transference to a vacuum-tube, it showed the spectrum of argon along with a spectrum resembling that of hydrocarbons. Having to leave off work at this stage, a short note was sent to the ‘Chemical News’ on a Possible Compound of Argon. On resuming work after the holidays, the gas was again investigated, and, on sparking with oxygen, carbon dioxide was produced. But it was thought right again to treat the gas with cuprous chloride in presence of ammonia, and it now appeared that when left for a sufficient time in contact with a strong solution, considerable contraction took place, carbonic oxide being removed. There can, therefore, be no doubt that, although apparently all gas had been removed from the carbon electrodes before admitting argon, some carbon dioxide must have been still occluded, probably in the upper part of the electrodes, and that the prolonged heating due to the arc had expelled this gas and converted it into monoxide. It was, indeed, inexplicable how an expansion should have taken place unless by some such means; for the combination of a monatomic gas must necessarily be accompanied by contraction. It appears, therefore, certain that argon and carbon do not combine, even at

* ‘Phil. Trans.,’ vol. 186, A.

the high temperature of the arc, where any product would have a chance of escaping decomposition by removing itself from the source of heat. It is hardly necessary to point out that such a process lends itself to the formation of endothermic compounds such as acetylene, and it was to be supposed that if argon is capable of combination at all, the resulting compound must be produced by an endothermic reaction.

2. A product rich in barium cyanide was made by the action of producer gas on a mixture of barium carbonate and carbon at the intense temperature of the arc. This product was treated by Dumas' process so as to recover all nitrogen; and, as argon might also have entered into combination, the nitrogen was absorbed by sparking. All the nitrogen entered into combination with oxygen and soda, leaving no residue. Hence it may be concluded that no argon enters into combination. For the successful carrying out of these experiments we have to thank Mr. G. W. MacDonald.

3. A mixture of argon with the vapour of carbon tetrachloride was exposed for several hours to a silent discharge from a very powerful induction coil. The apparatus was connected with a gauge which registered the pressure of the vapour of the tetrachloride and of the argon of which it was mixed. Careful measurement of the pressure was made before commencing the experiment, and after its completion. Although a considerable amount of other chlorides of carbon was produced, no alteration of pressure was noticeable; the liberated chlorine having been absorbed by the mercury present. Here again the argon did not enter into the reaction, but it was recovered without loss of volume.

The remaining experiments relate to attempts to produce compounds of helium. The plan of operation was to circulate helium over the reagent at a bright red heat, and to observe whether any alteration in volume occurred—an absorption of a few c.c. could have been observed—or whether any marked change was produced in the reagent employed. As a rule, after the reagent had been allowed to cool in the gas, all helium was removed with the pump, and the reagent was again heated to redness, so as, if a compound had been formed, to decompose it and expel the helium. Every experiment gave negative results; in no case was there any reason to suspect that helium had entered into combination.

A short catalogue of the substances tried may be given.

4. Sodium distilled in the current of gas, and condensed in drops with bright metallic lustre. The glass tube in which it was heated became covered with a coating of

5. Silicon, which caused no absorption.

6. A mixture of beryllium oxide and magnesium, yielding metallic beryllium, was without action.

7. Zinc and, 8, cadmium distilled over in the current of gas.

9. A mixture of boron oxide and magnesium dust, giving elemental boron, produced no absorption.

10. Similarly, a mixture of yttrium oxide and magnesium dust had no effect.

11. Thallium was heated to bright redness in the gas, retaining its metallic lustre.

12. Titanium oxide mixed with magnesium dust was heated to bright redness, and caused no absorption.

13. Similar absence of action was proved with thorium oxide and magnesium powder.

14. Tin and, 15, lead, were heated to bright redness in the current of gas, and remained untarnished.

16. Phosphorus was distilled in the gas, and caused to pass through a length of combustion-tube heated to softening. Some red phosphorus was formed, but no alteration of volume was noticed.

17. The same process was repeated with elemental arsenic.

18. Antimony and, 19, bismuth, at a bright red heat, retained their metallic lustre.

20. Sulphur and, 21, selenium, were treated in the same way as phosphorus; no action took place.

22. Uranium oxide, mixed with magnesium dust, was heated to bright redness in helium. No change, except the reduction of the oxide, took place. The mixture was allowed to cool slowly in the current, and the helium was removed with the pump till a phosphorescent vacuum was produced in a vacuum tube communicating with the circuit. The mixture was re-heated, and no helium was evolved—not even enough to show a spectrum. The vacuum remained unimpaired.

It had been hoped that elements with high atomic weight, such as thallium, lead, bismuth, thorium, and uranium might have effected combination, but the hope was vain.

23. A mixture of helium with its own volume of chlorine was exposed to a silent discharge for several hours. The chlorine was contained in a reservoir, sealed on to the little apparatus which had the form of an ozone apparatus. No change in level of the sulphuric acid confining the chlorine was detected after the temperature, raised by the discharge, had again become the same as that of the room. Hence helium and chlorine do not combine.

24. Metallic cobalt in powder does not absorb helium at a red heat.

25. Platinum black does not occlude it.

26. It is not caused to combine by passage over a mixture of soda-lime and potassium nitrate heated to bright redness. This was hardly to be expected, for it resists the action of oxygen in presence of caustic soda, even when heated by the sparks which traverse it.

27. A mixture of soda-lime and sulphur consisting of polysulphides causes no change of volume in a current of helium passed over it at a bright red heat.

28. Induction sparks in an ozone apparatus passed through a mixture of helium with benzene vapour in presence of liquid benzene for many hours, gave no change of volume. The benzene was, of course, altered, but the sum of the pressures of the helium and the benzene-vapour remained as at first. Had helium been removed, contraction would have occurred.

This ends the catalogue of negative experiments. Any compound of helium capable of existence will probably be endothermic, and the two methods of producing endothermic compounds, where no simultaneous exothermic reaction is possible, are exposure to a high temperature, at which endothermic compounds show greater stability, and the influence of the silent electric discharge. These methods have been tried, so far in vain. There is, therefore, every reason to believe that the elements, helium and argon, are non-valent, that is, are incapable of forming compounds.

“On the Amount of Argon and Helium contained in the Gas from the Bath Springs.”* By LORD RAYLEIGH, Sec. R.S. Received April 30,—Read May 21, 1896.

The presence of helium in the residue after removal of nitrogen from this gas was proved in a former paper,† but there was some doubt as to the relative proportions of argon and helium. A fresh sample, kindly collected by Dr. Richardson, has therefore been examined. Of this 2500 c.c., submitted to electric sparks in presence of oxygen, gave a final residue of 37 c.c., after removal of all gases known until recently. The spectrum of the residue, observed at atmospheric pressure, showed argon, and the D_3 line of helium very plainly.

The easy visibility of D_3 suggested the presence of helium in some such proportion as 10 per cent., and this conjecture has been confirmed by a determination of the refractivity of the mixture. It may be remembered that while the refractivity of argon approaches closely that of air, the relative number being 0.961, the refractivity of helium (as supplied to me by Professor Ramsay) is very low, being only 0.146 on the same scale. If we assume that any sample

* I am reminded by Mr. Whitaker that helium is appropriately associated with the Bath waters, which, according to some antiquaries, were called by the Romans *Aquæ Solis*.

† ‘Roy. Soc. Proc.’ vol. 59, p. 206, 1896.

of gas is a mixture of these two, its refractivity will determine the proportions in which the components are present.

The observations were made by an apparatus similar in character to that already described, but designed to work with smaller quantities of gas. The space to be filled is only about 12 c.c., and if the gas be at atmospheric pressure its refractivity may be fixed to about 1/1000 part. By working at pressures below atmosphere very fair results could be arrived at with quantities of gas ordinarily reckoned at only 3 or 4 c.c.

The refractivity found for the Bath residue after desiccation was 0.896 referred to air, so that the proportional amount of helium is 8 per cent. Referred to the original volume, the proportion of helium is 1.2 parts per thousand.

PRSL 60

"On the Changes produced in Magnetised Iron and Steels by cooling to the Temperature of Liquid Air." By JAMES DEWAR, LL.D., F.R.S., Fullerman Professor of Chemistry in the Royal Institution of Great Britain, and J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London. Received April 25,—Read May 21, 1896.

The action of the low temperature produced by liquid air upon the magnetic moment of steel magnets was studied by one of us in a few cases in a preliminary research made some time ago.* We have recently returned to the subject and made further investigations on the influence of the low temperatures thus obtained on magnetised iron and steels of very various compositions, with the object of determining the nature of the changes which take place in the magnetic moment of small magnets constructed of these metals, when cooled gradually or suddenly down to the lowest temperature obtainable by the use of boiling liquid air. The arrangements adopted in this investigation were as follows:—

A reflecting magnetometer consisting of three small magnetised needles of watch-spring steel, cemented to a concave glass mirror, suspended by a single cocoon fibre, was placed in a tube so as to be free from disturbance by draughts of air. The small magnets were 8 to 10 mm. in length. The image of a portion of the filament of an incandescent lamp was reflected by the mirror on to a divided scale placed at a distance of 70 cm. from the mirror. The edge of the very sharp image of the filament, focussed upon the scale,

* Friday evening discourse at the Royal Institution, "On the Scientific Uses of Liquid Air," by James Dewar, LL.D., F.R.S., January 19, 1894.

enabled any angular displacement of the magnetometer needle to be easily determined. The position of this magnetometer needle was regulated by the field produced by an external controlling magnet. The small magnet, the behaviour of which at low temperatures was to be studied, was placed behind the magnetometer, with its centre at a distance of 1 to 10 cm. from the centre of the magnetometer needle and its axis in a direction passing through the centre of the magnetometer needle, and at right angles to the direction of the undisturbed magnetometer needle. The magnet to be examined was fixed to a brass wire, held in a wooden support in such fashion that the magnet under examination could be easily removed from its position behind the magnetometer, and restored to it again exactly. A large number of samples of steel and iron were then prepared in the form of small needles, generally 15 mm. long and about 1 mm. in diameter. These steels comprised nickel steels, with various percentages of nickel; chromium steels, with various percentages of chromium; aluminium steels, with various percentages of aluminium; tungsten steels, manganese steels, silicon steel, ordinary carbon steels in various states of tempering, soft-annealed transformer iron, soft-iron wire, and the same irons hardened by hammering. For most of these samples of steels we were indebted to Mr. R. A. Hadfield, of Sheffield, who kindly furnished them to one of us in the form of wires.

These short steel magnets were then all magnetised to "saturation" by placing them for a few moments between the poles of a powerful electro-magnet. One by one they were then placed in position behind the magnetometer, and the deflection produced on the magnetometer needle observed. In any particular case this deflection may be taken as approximately representing the intensity of magnetisation of the sample, although, owing to the varying sizes of the sample and distance from the magnetometer, the deflections in the case of different magnets are not comparable with one another, and cannot be taken as indicating the relative intensities of magnetisation of two different samples. This, however, was not important, as our object was not to compare the absolute values of the magnetisation of different classes of steels, but to observe the mode of variation of the magnetisation of any one sample when cooled from ordinary temperatures down to the temperature of liquid air.

The method of proceeding was then as follows:—Having adjusted the image of the lamp filament to the zero of the scale, the small magnet under observation was placed behind the magnetometer, and the deflection of the magnetometer needle observed. A small vacuum-jacketed cup, filled with liquid air, was then brought up underneath the sample, and by its aid the magnet cooled suddenly *in situ* to a temperature in the neighbourhood of -186°C . In the

many cases this sudden cooling immediately deprived the magnet of a considerable percentage of its magnetisation, and the magnetic moment was reduced. This, however, was not universally the case. In some cases, as in that of the chromium steels, the first effect of this sudden cooling was an increase in the magnetic moment of the magnet; in other cases hardly any change in the magnetic moment at all. The vessel of liquid air was then removed, and the magnet allowed to heat up again, which it very quickly did, to the temperature of the room, or rather to a temperature at which the deposit of snow formed upon the needle immediately on coming out of the liquid air, fully melted. This was taken to be at about 5°C . It was found that each magnet had certain peculiarities of its own.

Taking first the ordinary carbon steel (a sample of knitting-needle steel) we observe the following facts:—

Knitting-needle Steel (a) Tempered Glass Hard.—The first effect of cooling this magnet was to diminish the magnetic moment by 6 per cent. On allowing the magnet to heat up again to the ordinary temperature, the magnetic moment still further diminished by about 16 per cent. On cooling again the magnetic moment increased 10 per cent., and from and after that time cooling the magnet always increased the magnetic moment, and allowing to heat up again to ordinary temperature always diminished the magnetic moment, the magnetic moment at -185°C . being about 10 per cent. greater than the magnetic moment at 5°C . The first effect, therefore, of the cooling was to permanently diminish the magnetic moment, but after a few alternations of heating and cooling, the magnet reached a permanent condition in which its moment, when cold, was greater than its moment when warm. These changes of magnetisation may be best represented as in the diagram in fig. 1, in which the firm lines represent to some arbitrary scale the moment of the magnet when at its ordinary temperature of 5°C ., and the dotted lines represent to the same scale the moment of the magnet when cooled to -185°C .

Knitting-needle Steel (b) Medium Temper.—The same general results were obtained with knitting-needle steel tempered to a medium temper. The first effect of the cooling to the low temperature was to diminish the moment of the magnet. On allowing it to heat up again the moment of the magnet diminished still more. The next cooling caused an increase of magnetic moment, and from and after that time the steel settled down into a permanent condition in which the magnetic moment was greater at -185°C . than at 5°C . by nearly 20 per cent. of its value at 5°C . (see fig. 2).

Knitting-needle Steel (c) Annealed Soft.—The same general course of events was noticed in the case of the knitting-needle steel when made soft by heating to a red heat and allowing it to cool very

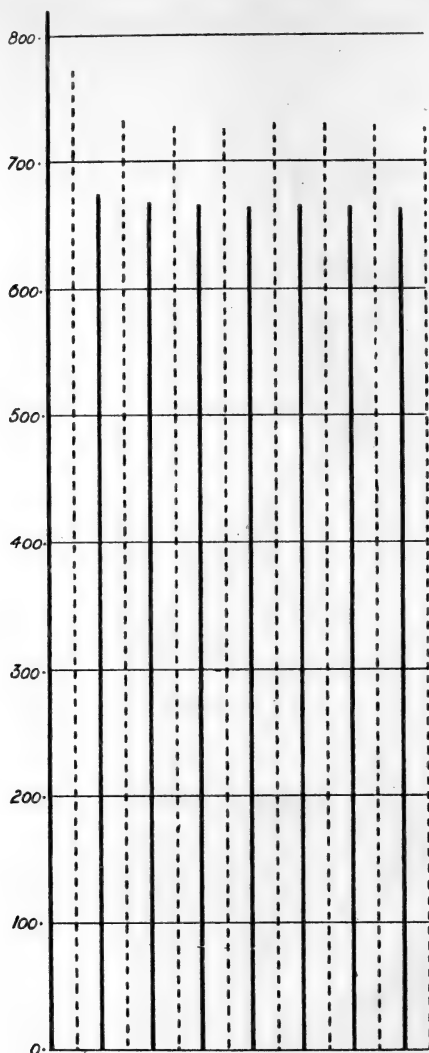


FIG. 1.—Knitting-needle steel (glass hard).

slowly. In this case, however, the first diminution of magnetic moment was still greater. On first immersion in the liquid air the magnet lost about 33 per cent. of its moment. On allowing it to heat up again to 5°C . it still further diminished in moment, and from and after that point it arrived soon at a permanent condition, in which its moment, when cold, was greater than its moment when warm by 30 per cent. of its moment at 5°C . These

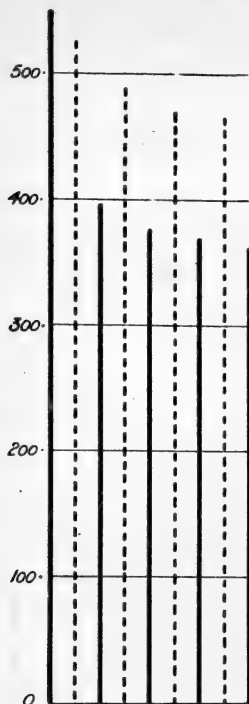


FIG. 2.—Knitting-needle steel (medium temper).

changes of the medium- and soft-tempered steel are represented by the lines in the diagrams 2 and 3, in which the firm lines are proportional to the magnetic moment of the magnet at $5^{\circ}\text{C}.$, and the dotted lines proportional to the magnetic moment at $-185^{\circ}\text{C}.$ It will be seen that, in the case of this carbon steel, the effect of softening the steel is to make more pronounced the effect of the final temperature changes; the change of moment caused by cooling from the ordinary temperature to the temperature of liquid air, when the permanent condition has been reached, being in the case of the glass-hard steel an increase of magnetic moment of about 12 per cent.; in the case of the same steel with a medium temper about 22 per cent., and in the case of the same steel tempered very soft about 33 per cent. (see fig. 3).

Chromium Steels.—Observations were then made with the magnets of chromium steel, having respectively 0.29 per cent., 1.18 per cent., 5.44 per cent., and 9.18 per cent. of chromium. In all these cases the first effect of cooling the magnet was to cause at once an increase of magnetic moment, and the subsequent heating up again to the ordinary temperature caused a decrease of magnetic moment. These

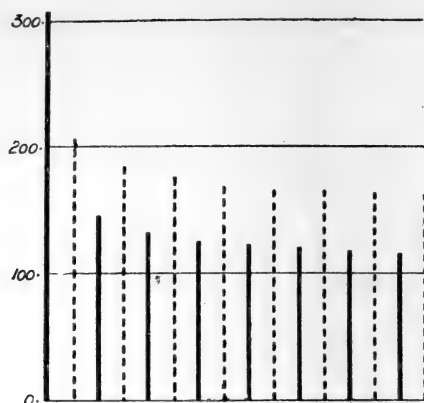


FIG. 3.—Knitting-needle steel (tempered soft).

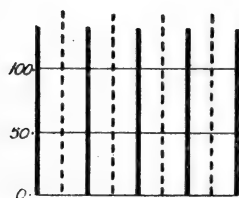


FIG. 4.—Chromium steel.

Cr = 0·29
C = 0·16
Si = 0·07
Mn = 0·18
Fe = 99·30

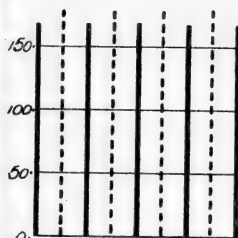


FIG. 5.—Chromium steel.

Cr = 1·18
C = 0·27
Si = 0·12
Mn = 0·21
Fe = 98·22

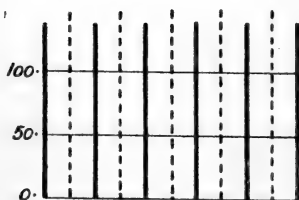


FIG. 6.—Chromium steel.

Cr = 5·44
C = 0·27
Si = 0·50
Mn = 0·61
Fe = 92·68

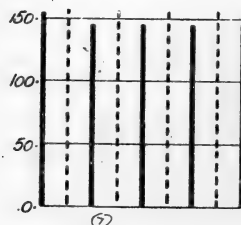


FIG. 7.—Chromium steel.

Cr = 9·18
C = 0·71
Si = 0·36
Mn = 0·25
Fe = 89·50

magnets arrived almost immediately at their permanent condition, in which the magnetic moment, when cold, was greater than the magnetic moment when warm by about 12 per cent. The variation of magnetic moment in the case of these magnets is shown by the diagrams 4, 5, 6, and 7, in which the firm lines represent the magnetic moment when the magnet is at 5°C ., and the dotted lines the magnetic moment at -185°C . It will be seen, therefore, that in the case of the magnets there was no such initial decrease of magnetisation as in the case of the carbon steel magnets. The analysis of these steels was furnished to us by Mr. Hadfield, and is appended to the diagrams. These steels are all in their hard condition, and possess considerable coercive force.

Aluminium Steels.—The aluminium steels employed had the following percentages of aluminium, viz.: 0.72, 1.16, and 1.60. In all these cases the first effect of cooling the magnet made of these steels was to cause a very small diminution in the magnetic moment, but not more than about 2 per cent. (see figs. 8, 9, and 10). The subsequent rise in temperature of the magnet again to its ordinary tem-

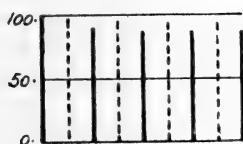


FIG. 8.—Aluminium steel.

Al = 0.72
C = 0.20
Si = 0.12
Mn = 0.11
Fe = 98.85

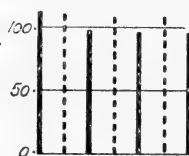


FIG. 9.—Aluminium steel.

Al = 1.16
C = 0.26
Si = 0.15
Mn = 0.11
Fe = 98.32

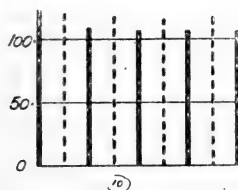


FIG. 10.—Aluminium steel.

Al = 1.60
C = 0.21
Si = 0.18
Mn = 0.18
Fe = 97.83

perature, caused a still further fall in magnetic moment, and from and after that point the effect of cooling down to the temperature of

liquid air was to cause the magnet to possess a magnetic moment about 10 per cent. greater at -185°C. than at 5°C. It will be seen, therefore, that these steels differ from the chromium steels in this respect, that whereas in the chromium steels the effect of the first cooling is to cause an increase in magnetic moment; in the case of the aluminium steels, the effect of the first cooling was to cause a decrease of magnetic moment, although much smaller relatively than in the case of the carbon steels.

Nickel Steels.—Experiments were then made with samples of nickel steel containing 0.94, 3.82, 7.65, 19.64, and 29 per cent. of nickel. These steels exhibited some rather interesting peculiarities. In the case of the nickel steel with 0.94 per cent. of nickel, the effect of the first cooling in liquid air was to cause a very small decrease in magnetic moment (see fig. 11), and the subsequent heating and cooling

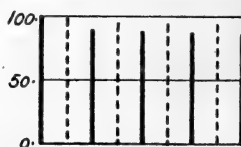


FIG. 11.—Nickel steel.

Ni = 0.94
C = 0.13
Si = 0.23
Mn = 0.72
Fe = 97.98

brought the steel into a condition in which its magnetic moment, when cold, was always greater than its magnetic moment when warm, by about 10 or 11 per cent. In the case of the nickel steel with 3.82 per cent. of nickel, the effect of the changes of temperature was very similar (see fig. 12), and also in the case of the nickel steel having 7.65 per cent. of nickel the order of the changes was not very different—in this respect, that the magnetic moment when cold was

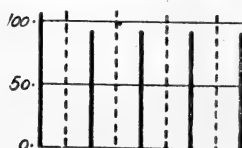


FIG. 12.—Nickel steel.

Ni = 3.82
C = 0.19
Si = 0.20
Mn = 0.65
Fe = 95.14



FIG. 13.—Nickel steel.

Ni = 7.65
C = 0.17
Si = 0.28
Mn = 0.68
Fe = 91.22

greater than the magnetic moment when warm, when the permanent state had been reached. But it will be noticed from the diagrams (see fig. 13) that in the case of the 7.65 per cent. nickel steel, the effect of the first cooling was to cause a slight increase in magnetic moment. A remarkable peculiarity, however, was found in the case of the 19.64 per cent. nickel steel. In this case the effect of the first cooling was to cause a very considerable reduction of magnetic moment, very nearly 50 per cent., that is to say, the magnetic moment fell instantly, on cooling in the liquid air, to about half the value that it had at the beginning of the experiment. On taking the magnet out of the liquid air and allowing it to warm up again to the temperature of the room, the magnetic moment immediately *increased* again, and from and after that time the effect of the temperature change on the magnetic moment was such that the magnetic moment, when cooled to the temperature of liquid air, was always *less* than the magnetic moment at 5° C. by about 25 per cent. of the latter value. These relative changes are shown in the diagram (fig. 14). These experiments

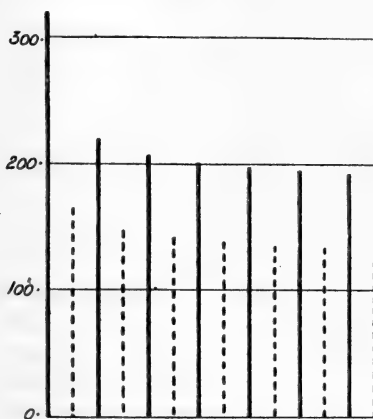


FIG. 14.—Nickel steel.

Ni = 19.64

C = 0.19

Si = 0.27

Mn = 0.93

Fe = 78.97

100.00

with the 19 per cent. nickel steel were repeated a great many times, and always with the same general results. The sample of 29 per cent. nickel steel was then examined, and it was found that the magnetic changes produced in it on heating and cooling were of the same general character as in the case of the 19 per cent. sample, only less

marked. Steels having these high percentages of nickel are, as Dr. J. Hopkinson has pointed out,* remarkable for the wide range of temperature within which they can exist in two states, one considerably magnetic, and one practically non-magnetic or but feebly magnetic. In these two states their mechanical and other physical properties are entirely different. In the experiments here mentioned, the nickel steel samples were in the magnetic condition. They are put into this condition by dipping for one moment in liquid air, and are only transformed back into the feebly magnetic condition by heating to a cherry-red heat. The 29 per cent. sample of nickel steel being in the magnetic condition was magnetised by contact with the poles of the electromagnet. On cooling it in liquid air it immediately lost about 20 per cent. of its moment, on warming up again to 5° C. it lost about 5 per cent. more, and from and after that point remained in a condition in which cooling the magnet to -185° C. caused its moment to become about 10 per cent. less than it was at 5° C. Hence the 29 per cent. nickel steel exhibits the same quality but in a less marked degree than the 19 per cent., in that its magnetic moment is decreased by cooling to -185° C., and recovers again on heating up to 5° C. In this respect the two samples of nickel steel differ from all other samples of steel which we have examined, in that they have a negative temperature coefficient for magnetic moment change with temperature, after the first change on cooling has taken place.

Pure Nickel.—In order to see if this peculiarity extended to pure nickel, we examined the behaviour of a small magnet made with Mr. Mond's pure nickel, but we found that such a nickel magnet, magnetised to saturation, behaved exactly as did a carbon steel magnet (see fig. 15). The effect of the first cooling to the temperature of liquid air was to diminish the magnetic moment. On allowing the magnet to heat up again to the ordinary temperature the moment diminished still more, and from and after that time the behaviour of the magnet was perfectly normal, that is to say, its magnetic moment when at 5° C. was less than its magnetic moment at -186° C., but only by about 3 or 4 per cent. of the latter value.

Silicon Steel.—A sample of silicon steel, containing 2.67 per cent. of silicon, behaved in a normal manner (see fig. 16). The magnet experienced a permanent diminution of moment on cooling for the first time, and after that, its magnetic moment when cold was greater than its magnetic moment when warm.

Soft Iron.—In order to determine if similar changes of magnetic moment could be produced in the case of soft annealed iron, small magnets of Swedish iron were prepared, formed of a short length, about 15 mm., of soft iron, or a small slip of annealed transformer iron. On magnetising these in a strong field, and testing them with

* 'Roy. Soc. Proc.,' 1890, vol. 47, p. 138.

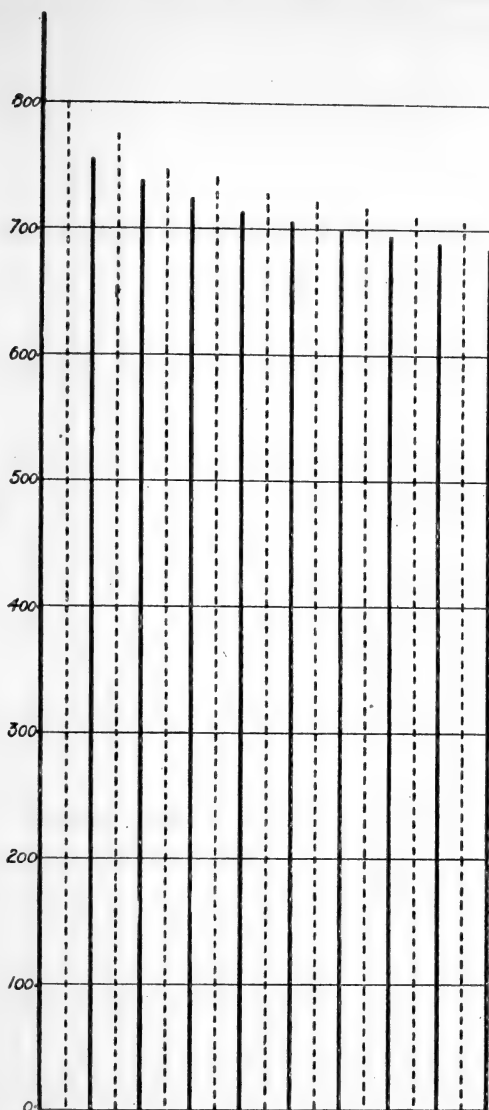


FIG. 15.—Mond's pure nickel.

the magnetometer, and cooling them by immersing in liquid air, it was found that the first effect of the cooling was to produce a small diminution in the magnetic moment, and the subsequent heating in some cases produced a further diminution of magnetic moment. In the first sample of soft iron, the wire was about 3 cm. long, and bent

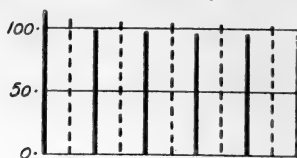


FIG. 16.—Silicon steel.

Si = 2.67
 C = 0.20
 Mn = 0.25
 Fe = 96.88.

into a U shape, with ends about 10 mm. apart, and in this case the changes of magnetic moment, as shown in fig. 17, were similar to

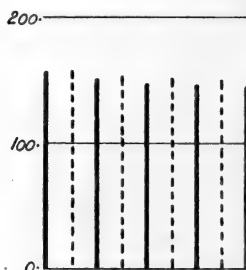


FIG. 17.—Soft iron.

those in the case of the carbon steels, only with very much narrower limits of variation. The first cooling hardly produced any change whatever in the magnetic moment of the magnet. On allowing it to heat up again, the magnetic moment was very slightly diminished, and thenceforth the changes of moment were such that the magnetic moment was greater when the magnet was cold than when it was warm, by about 2 or 3 per cent. of the latter value. In the case of a straight, soft iron magnet, formed of annealed transformer iron, the curious fact was noticed that whereas a rapid cooling of the magnet by plunging into liquid air hardly produced any effect on the magnetic moment after the first initial loss of magnetism had taken place on cooling, the effect of a slow cooling down to the temperature of -185°C. was always to produce a permanent diminution of magnetic moment. Hence the magnetism of this soft iron sample could be frittered away by a process of slow cooling to -185°C. , and intermediate heating up to 5°C. These changes of moment are represented in the diagram of fig. 18.

Hard Iron.—A sample of the same iron, hardened by hammering, was tested, and was found to behave in a very similar manner to the

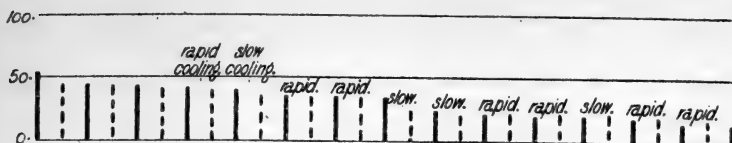


FIG. 18.—Annealed transformer iron.

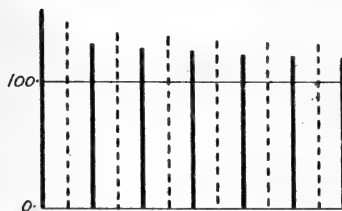


FIG. 19.—Hard transformer iron.

glass-hard carbon steel (see fig. 19), the changes in magnetic moment being relatively about the same percentage for the same temperature change: that is to say, the magnet had a moment of about 10 per cent. greater at -185°C. than at 5°C.

A series of tungsten steels were then examined, having respectively 1, 7.5, and 15 per cent. of tungsten in them.

Magnets were prepared of these steels, both in the glass-hard condition and in a carefully annealed condition. These steels were found to resemble the simple carbon steels in that the first effect of cooling the magnet to -186°C. was to cause a diminution of magnetic moment, and the subsequent warming up again to 5°C. , a still further decrease in magnetic moment. From that time forth cooling the magnet always caused an increase of magnetic moment. The effect of increasing the percentage of tungsten was to cause a decrease in the variation of the magnetic moment over a given temperature range. That is to say, the hardened 15 per cent. tungsten steel temporarily lost magnetic moment to the extent of about 6 per cent. by heating up from -185°C. to 5°C. when once the initial condition had been passed. The 7.5 per cent. tungsten steel lost moment to the extent of about 10 per cent., and the 1 per cent. tungsten steel lost moment to the extent of about 12 per cent. when the temperatures rose between the same limits. As regards these tungsten steels, softening the steel caused the magnetic moment to decrease by a greater percentage when heated up from -185°C. to 5°C. than was found to be the case when the steel was in its hard condition. A sample of manganese steel containing 12 per cent. of manganese was rendered magnetic by heating for 24 hours to a dull red heat. A small magnet prepared from this steel was found capable of retaining

magnetism. On cooling it to -185°C. , it slightly increased in magnetic moment, and on heating up again to 5°C. , its magnetic moment decreased to the extent of about 3 per cent. of its moment at -185°C. There was no initial decrease of moment in this case. In this respect, therefore, it resembled the chromium steel magnets.

Broadly speaking, the results so far obtained are:—

(1) That the sudden cooling to the temperature of liquid air usually permanently decreases the magnetic moment of short magnets made of many varieties of steel, assuming them to have been initially magnetised in a strong field.

(2) This initial decrease is found both in hardened steels having great coercive force, and also in the same steels in a soft or annealed condition, and is especially conspicuous in the case of the 19 per cent. nickel steel.

(3) In the case of most steels so far examined, the effect of cooling magnets made of them to -185°C. is to temporarily increase the magnetic moment after the permanent magnetic condition has been reached.

(4) The exceptions to the above rule so far noted are the nickel steels with percentages of nickel from 19 to 29 per cent., in which case the magnetic moment is always decreased temporarily by cooling to -185°C. , after the permanent magnetic condition has been reached.

(5) It appears from these experiments that one of the best ways of ageing a permanent magnet is to dip it several times into liquid air. It then arrives at a constant condition in which subsequent temperature changes have a definite effect, and in which the subpermanent magnetism is removed.

Note added May 4.

Since the 19 per cent. nickel steel magnet increases in magnetic moment when heated from -185°C. to $+5^{\circ}\text{C.}$, and since it is well known that at some higher temperature it would lose magnetic moment altogether, it was considered very desirable to ascertain the temperature at which it would have its maximum magnetic moment. The magnet was accordingly heated (on April 2) in an oil bath gradually up to a temperature of about 300°C. , and the deflections of the magnetometer observed at intervals, both as the temperature rose and as it fell. The result showed that this nickel steel magnet continued to increase in magnetic moment, until a temperature of about 30°C. was reached, and the magnetic moment then began to decrease.

At a temperature of $+300^{\circ}\text{C.}$, the moment of the magnet was not much greater than it was at -185°C. On cooling down again from

300° C., the moment increased, but not to the same maximum as before; and on repeating the cycle of temperature from about 15° C. to 300° C., the magnetic moment gradually varied, in the manner shown in fig. 20, and the temperature of maximum magnetic moment

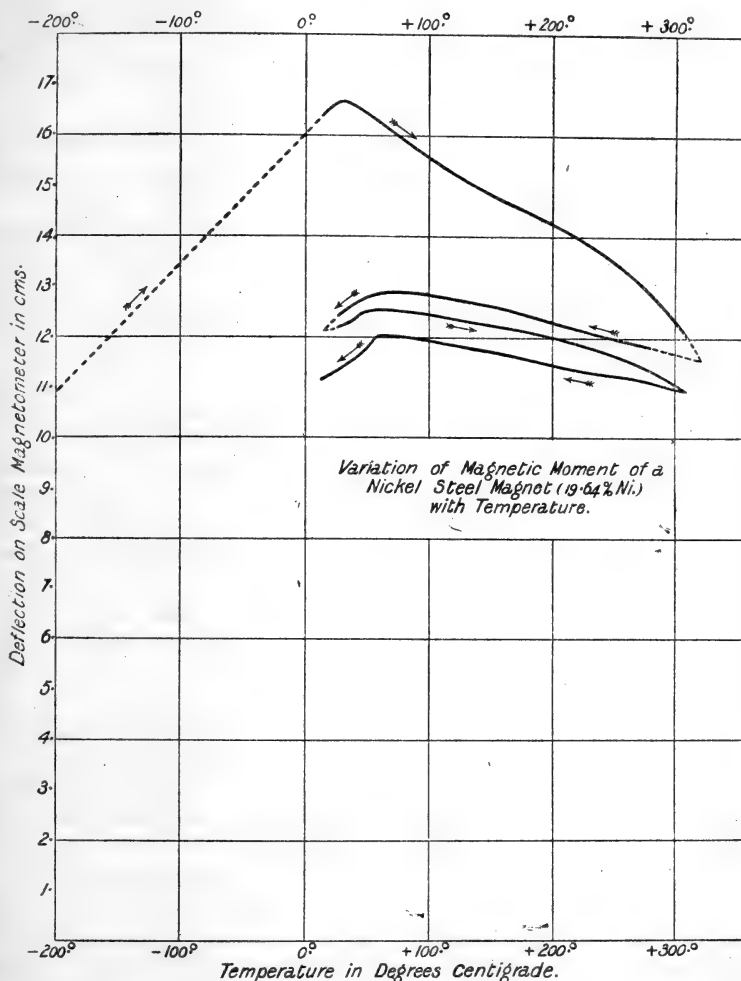


FIG. 20.

gradually shifted upwards to about 56° C. This magnet is, therefore, an interesting case of a sample of steel which, when magnetised, has a maximum magnetic moment at a certain temperature.

PpSL 60

“On the Electrical Resistivity of Bismuth at the Temperature of Liquid Air.” By JAMES DEWAR, LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, and J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London. Received May 19,—Read June 4, 1896.

In the course of last year we published some observations (see ‘Phil. Mag.,’ September, 1895, p. 303)* on the electrical resistance of bismuth at the temperatures of liquid and solid air, in which the resistivity of certain samples of bismuth was measured at various temperatures down to the temperature at which air solidifies. These observations showed some anomalous results. In the case of two samples of bismuth used by us, and prepared by different chemical means, it was found that the resistivity reached a minimum value at a temperature of about -80° , and that after that point further cooling increased the electrical resistivity of these samples of the metal. In the case of another sample of commercial bismuth, the resistivity curve was a curve of double curvature. These results, together with the high absolute value of the resistivity of the samples, caused us to feel a strong conviction that different results would be obtained with bismuth prepared by an electrolytic method. Some observers, particularly M. van Aubel, who have investigated the electrical properties of bismuth, have expressed the opinion that bismuth cannot be prepared in a state of perfect purity by any chemical means. Finding the chemical methods of doubtful utility, we accordingly solicited the assistance of Messrs. Hartmann and Braun, who have devoted a large amount of attention to the preparation of pure electrolytic bismuth for the purposes of constructing spirals of bismuth for measuring the strength of magnetic fields. They kindly prepared for us at our request a considerable quantity of bismuth by an electrolytic method, which examination showed to be exceedingly pure, and this metal was pressed into a uniform wire with a diameter of about half a millimetre. This electrolytic bismuth is very soft, and in the form of wire can be bent without difficulty. Resistance coils were accordingly constructed of this wire, of a form suitable for use when measured in liquid air and at low temperatures. In the case of one resistance coil, which may be denoted as electrolytic bismuth No. 1, the length of the wire employed was 80.85 cm.; the diameter of this wire was carefully measured with a microscopic

* “The Variation in the Electrical Resistance of Bismuth when cooled to the Temperature of Solid Air,” Dewar and Fleming, ‘Phil. Mag.,’ September, 1895, p. 303.

micrometer in twenty to thirty places, these diameters having very nearly equal values, and a mean value of 0.05245 cm. The bismuth wire so prepared was mounted on a suitable holder, and its resistance was taken at several different temperatures and in liquid air, the temperatures being in all cases measured by our standard platinum thermometer P₁.*

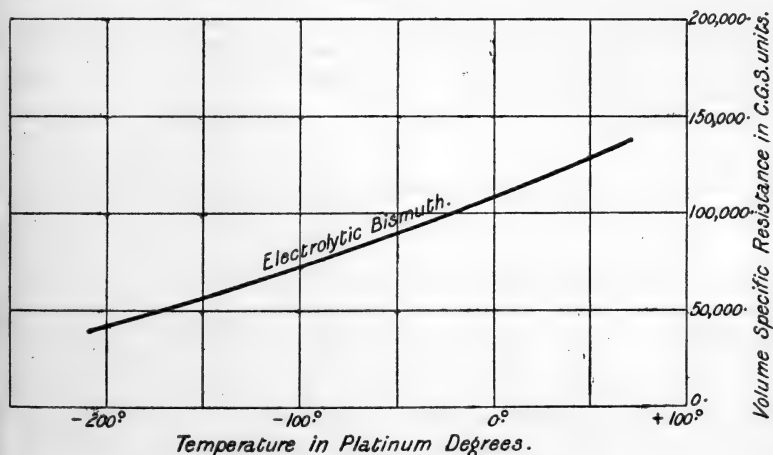
The results of these measurements were as follows:—

Resistivity of Electrolytic Bismuth. No. I.

Temperature in platinum degrees.	Observed resistance in ohms.	Resistivity in C.G.S. units per cubic centimetre.	Remarks.
+60°·5	4.9857	133250	At ordinary temperature.
+19°	4.3464	116180	
−61°·2	3.1275	83590	In ether cooled with solid carbonic acid.
−202°·2	1.5256	40780	In liquid air.

The curve of resistivity plotted from these data is shown in fig. 1, and in the table the value of the resistivity of bismuth in C.G.S. units per cubic centimetre is given above. These values of the resistivity show that in the case of this pure electrolytic bismuth

FIG. 1.



* For details of this thermometer, see Dewar and Fleming on the "Thermoelectric Powers of Metals and Alloys at the Boiling Point of Liquid Air," 'Phil. Mag.,' July, 1895, p. 100.

there is no tendency of the resistivity curve to a minimum value. Down to the lowest temperatures reached in these experiments, the resistivity of bismuth continues to decrease in a perfectly regular manner, and in such a way as to show that it would be no exception, in all probability, to the ordinary law, that resistivity of pure metals vanishes at the absolute zero of temperature. On comparing the results of these measurements with those in the former experiments made with chemically prepared bismuth, it is seen that the electrolytic bismuth used by us has a very much lower resistivity at 0°C. , viz., 108,000 units, and it has a lower value than that given by Matthiessen for pure bismuth, which is 129,700. We have, then, an additional indication that the bismuth used by us in the experiments in 1895 must have contained sufficient, though slight, impurity to markedly alter its resistivity, and to change entirely the character of the resistivity curve. With this electrolytic bismuth we have repeated the experiments which we made last year, on the variation of the electrical resistance of bismuth when placed transversely to the direction of the force in a magnetic field, and when cooled to the temperature of liquid air. For this purpose we constructed a flat spiral of the electrolytic bismuth, so arranged that its resistances could be measured at ordinary temperatures, and at the temperature of liquid air, by immersing it in a flat vacuum-jacketed test-tube, both when in a powerful magnetic field, and when merely in the terrestrial field. With this electrolytic bismuth we have confirmed the observation which we made last year, with a small sample of electrolytic bismuth, viz., that the effect of a given transverse magnetic field in increasing the resistivity of bismuth is immensely increased by cooling the bismuth to the temperature of liquid air. The figures in the following table will show the actual results obtained in these last experiments:—

Variation of Electrical Resistance of Electrolytic Bismuth in
Magnetic Fields of different Strengths.

Temperature in platinum degrees.	Magnetic field strengths in C.G.S. units.			Remarks.
	Zero.	1400 units.	2750 units.	
	Resistance of bismuth coil.			
+ 20°	ohms. 1·679	ohms. 1·700	ohms. 1·792	At ordinary temperature.
−202°	0·5723	1·4435	2·6801	In liquid air.

It will thus be seen that whereas the immersion of the electrolytic bismuth wire, at ordinary temperatures, transversely in a magnetic field of strength 2,750 C.G.S. units, only increased its resistance by about 6 per cent., the immersion of the same wire in the same magnetic field increased its resistance to more than four and a half times when it was cooled to the temperature of liquid air, and the effect of the cooling with liquid air is more than nullified by the field, so that the bismuth cooled in liquid air and at the same time placed in the field has a resistance of 50 per cent. greater than it was when not cooled and not in the field. We are engaged in extending these observations to stronger fields.

The behaviour of electrolytic bismuth in fields of various strengths and at various temperatures, from 0° C. to 100° C., has been studied by Mr. J. B. Henderson (*see* 'Phil. Mag.,' vol. 38, 1894, p. 488), and he has given a series of curves showing the variation of resistance of bismuth between these temperatures for fields of strength varying from zero to 22,700 C.G.S. units. Our observations at low temperatures are quite consistent with Mr. Henderson's curves. His curves indicate that at lower temperatures the effect of any given field in increasing the resistance of the bismuth becomes more marked.

Pressed to its limit it would appear that pure bismuth, which would in all probability be made a perfect conductor by reducing to the absolute zero of temperature, would be then converted into a non-conductor if at the same time immersed in a magnetic field of sufficient strength. Both M. van Aubel and Mr. Henderson have pointed out that the temperature coefficient of bismuth at any given temperature is quite altered by placing it in a magnetic field, and it will therefore be a matter of great interest to examine the effect of an exceedingly strong magnetic field as bismuth when cooled to the temperature of solid air.

By enclosing a bismuth wire and a platinum thermometer wire in the same mass of paraffin wax we have been able to measure the variation of resistance of the bismuth from the temperature of liquid air up to ordinary temperatures at a number of intermediate points, and to determine the resistance both in a zero magnetic field and in one of known strength, but the results we wish to reserve until we have had the opportunity of repeating them with stronger magnetic fields.

PRSL 60

“On the Electrical Resistivity of Pure Mercury at the Temperature of Liquid Air.” By JAMES DEWAR, LL.D., F.R.S., Fullerman Professor of Chemistry in the Royal Institution, and J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London. Received May 19,—Read June 4, 1896.

Although the electrical resistivity of mercury at ordinary temperatures has been carefully examined by many observers, and accurate determinations made of the specific resistance* and temperature coefficient, and in addition an examination made of the variation of resistivity in mercury when cooled to temperatures as low as $-100^{\circ}\text{C}.$ † we considered it would be of interest to examine the behaviour of pure mercury in respect of change in electrical resistivity when cooled to the temperature obtained by the employment of boiling liquid air. With this object we prepared a sample of very pure mercury in the following manner: Ordinary distilled mercury was shaken up with nitric acid in the usual manner to free it from other metals, and then carefully dried. It was then introduced into a bent glass tube formed of hard glass. This bent tube had both ends sealed, and a side tube connected in at the bend, by which it could be connected to a mercury vacuum pump. Two or three hundred grammes of the mercury was then introduced into one bend, and a high vacuum made in the tube. The side tube was then sealed off from the pump, and the mercury distilled over from one leg into the other. For this purpose, one leg of the bent tube was placed in ice and salt, and the other submitted to a gentle heat just sufficient to make the mercury distil under reduced pressure without ever bringing it into active ebullition. In this way the mercury is distilled over at a very low temperature, and the portion condensing in the cooler limb of the bent tube is entirely free from any contamination with silver, lead, zinc, or tin. By performing this distillation two or three times successively on the same mercury, a small quantity of mercury is at last obtained in an exceedingly pure condition. A glass spiral tube was then formed of lead glass, consisting of a tube having an internal diameter of about 2 mm., and a length of about 1 metre. This tube was bent into a spiral of about twelve close turns, each turn being nearly 2.5 cm. in diameter, and the ends of this spiral provided with enlarged glass ends formed of wider tube. The spiral,

* “The Specific Resistance of Mercury,” by Lord Rayleigh and Mrs. Sidgwick (*Phil. Trans. R. S.*, Part I, 1883). See, also, Mr. R. T. Glazebrook (*Phil. Mag.*, Oct., 1885), for other values.

† Cailletet and Bouty (*Compt. Rend.*, 100, 1188, 1885).

after being cleaned, was then very carefully filled with the purified mercury, and by running the mercury through a spiral several times, all air bubbles and air film were finally removed. Into the wider ends of the spiral, amalgamated copper electrodes were introduced, consisting of copper wire 4.4 mm. in diameter; the wider terminal ends of the spiral were then closed by paraffined corks to keep the copper electrodes in position. This spiral, full of mercury, was placed in a test-tube, and paraffin wax cast round it so as to enclose it entirely, leaving only the copper electrodes protruding. In order to determine the temperature of the mercury in the glass spiral tube, a platinum wire, the resistance of which was known at all temperatures down to the temperature of liquid air, was also embedded in the paraffin wax closely in contact with the glass spiral, and proper electrodes brought out to enable the resistance of this platinum wire to be determined. This mass of paraffin wax was then cooled down in a vacuum vessel kept filled up with liquid air until the whole mass reached the temperature of the liquid air. The glass spiral and thermometer enclosed in wax was then removed from the bath of liquid air and placed in a vacuum-jacketed test-tube, in order that it might warm up with extreme slowness to the ordinary temperature of the air.

Having in this manner cooled the mass of paraffin enclosing the glass spiral filled with mercury and the platinum resistance wire entirely to the temperature of liquid air, a series of observations were taken with the aid of two observers, one measuring the resistance of the mercury by a Wheatstone's Bridge, while at the same time the other observer at another slide wire bridge measured the resistance of the platinum wire, these observations being taken quite simultaneously, and continued whilst the mass heated up from -197.9° (platinum temperature) to 0° . All proper corrections were then applied to correct for the resistance of the connecting wires and the bridge temperature; and the observed resistance of the platinum wire employed was corrected to determine from its resistance temperatures in terms of the standard platinum thermometer employed by us in our investigations on the thermo-electric power of metals and alloys (see Dewar and Fleming, 'Phil. Mag.,' July, 1895, p. 95). This standard thermometer has always been denoted by the letter P_1 . The following table shows the corrected resistance of the mercury column and the corresponding platinum temperatures, as also the specific resistance of the mercury calculated from the accepted resistivity at 0° C. :—

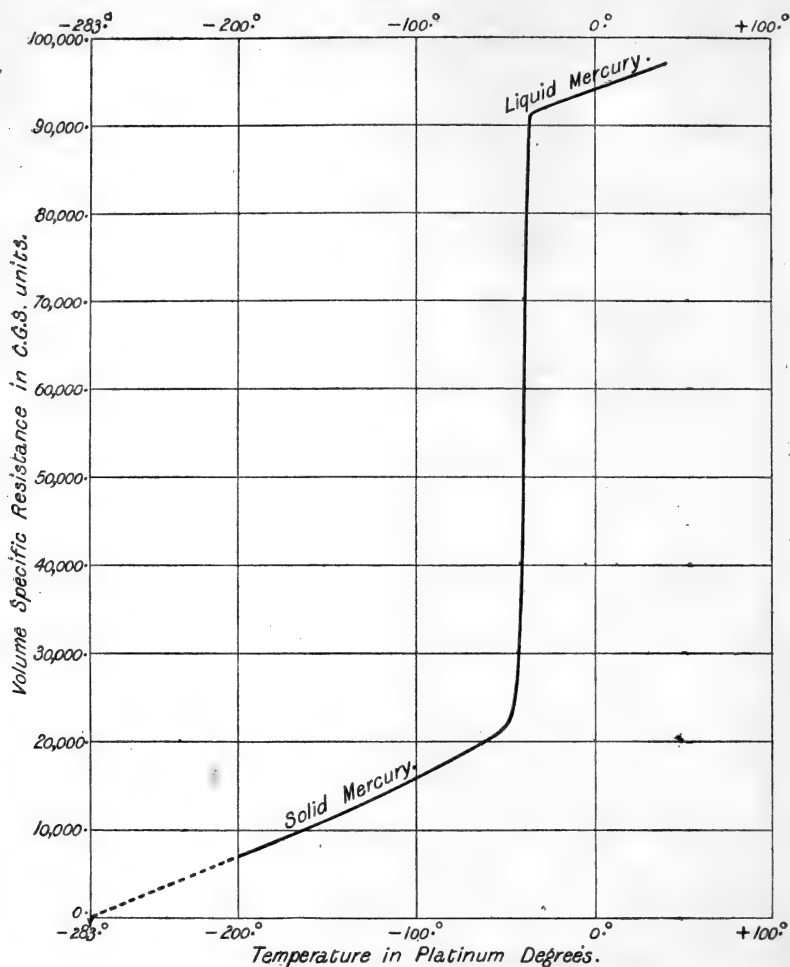
Resistivity of Pure Mercury in C.G.S. Units at various Temperatures in Platinum degrees.

Platinum temperature, <i>pt</i> , in terms of the standard platinum thermometer P_1 .	Observed and corrected resistance of mercury in lead glass spiral in ohms.	Resistivity of mercury in glass in C.G.S. units.
-197·9	0·0551	6970
-197·8	0·0551	6970
-197·5	0·0551	6970
-196·9	0·0566	7160
-195·2	0·0581	7350
-191·2	0·0601	7600
-182·7	0·0641	8100
-173·2	0·0721	9120
-168·4	0·0761	9620
-165·1	0·0781	9870
-157·4	0·0836	10570
-149·7	0·0886	11200
-143·0	0·0931	11770
-131·9	0·1011	12780
-128·3	0·1041	13160
-122·9	0·1081	13670
-117·5	0·1121	14170
-108·4	0·1191	15060
-103·7	0·1231	15560
- 97·0	0·1281	16200
- 91·1	0·1331	16830
- 85·0	0·1381	17460
- 79·1	0·1432	18100
- 73·1	0·1482	18740
- 67·4	0·1532	19370
- 63·2	0·1582	20000
- 57·6	0·1632	20630
- 52·5	0·1682	21270
- 48·9	0·1753	22160
- 47·0	0·1833	23180
- 46·0	0·1883	23810
- 44·9	0·1933	24440
- 44·2	0·1983	25070
- 43·5	0·2033	25700
- 43·0	0·2183	27600
- 42·4	0·2283	28860
- 42·1	0·2383	30130
- 41·9	0·2484	31410
- 41·2	0·2584	32670
- 40·8	0·2784	35200
- 40·6	0·2884	36460
- 40·4	0·3184	40260
- 39·7	0·3585	45330
- 39·5	0·3885	49120
- 39·4	0·4185	52920
- 39·3	0·4385	55440
- 39·1	0·4785	60800
- 38·7	0·5186	65570
- 38·5	0·5486	69360
- 38·3	0·5786	73160
- 37·7	0·6086	76950

Platinum temperature, <i>pt.</i> , in terms of the standard platinum thermometer P_1 .	Observed and corrected resistance of mercury in lead glass spiral in ohms.	Resistivity of mercury in glass in C.G.S. units.
- 37·6	0·6387	80760
- 37·2	0·6587	83280
- 36·7	0·6787	85810
- 36·0	0·7087	89600
- 35·2	0·7208	91140
- 33·7	0·7228	91380
- 31·2	0·7248	91640
0	0·7440	94070
+ 13·1	0·7518	95060
+ 16·3	0·7540	95330
+ 35·4	0·7653	96760

Adopting the value for the specific resistance of pure mercury at 0°C. , which has been recommended by the Board of Trade Electrical Committee, viz., 94,070 C.G.S. units, we have reduced the observed resistances of the mercury column at various temperatures to their equivalents in resistivity in absolute units, and placed these numbers against the observed resistances in the table above. As the specific resistance of mercury has been so carefully observed by many observers, we did not, for a moment, consider it necessary to attempt a further determination of this constant. On plotting out these values of the resistivity of mercury in the form of a curve in terms of the corresponding platinum temperatures, we find the resistivity curve has the form shown in fig. 1. It will be noticed that the resistivity of the mercury decreases gradually from the point at which the observations finished, viz., at $+35^{\circ}\text{C.}$, to the temperature -36° on the platinum scale. At this point the resistivity rapidly decreases to about one-quarter of its value in falling from -36° to -50° , and this sudden change all takes place within the range of about 14° of temperature. At the temperature of -50° on the platinum scale the resistivity curve again changes direction, and continues downwards in such a direction as to show that if produced along the same line from the lowest temperature actually observed, viz., -204° on the platinum scale, it would pass exactly through the absolute zero of temperature on this scale, which is -283°pt. It is also interesting to note that the part of the curve which corresponds to the mercury in the liquid state is almost exactly parallel to that part of the curve which corresponds to mercury in the solid condition, although, owing to the difference in the absolute values of the resistivities at these parts, the temperature coefficients as usually defined are very different. In the solid condition between the temperatures of $-197\cdot9^{\circ}$ and -97° , the mean increase in resistivity is

FIG. 1.



93.14 C.G.S. units per degree rise of temperature on the platinum scale; between -108.4° and -57.6° the mean increase in resistivity in C.G.S. units per degree is 109.6; in the liquid condition between the temperature -35.2° and 0° the mean increase in resistivity in C.G.S. units per degree is 83.2; temperature measurement being on the platinum scale as above defined. It may be stated here that temperatures defined by this platinum scale do not differ by more than about 0.5° from the Centigrade scale down to temperatures of -100° , but that the temperature of boiling liquid oxygen which, on the Centigrade scale is denoted by -182° , is, on the platinum scale

derived from our standard thermometer, denoted by -196.7° . This would show, therefore, that the temperature coefficient as usually defined is 0.000884 between -35° and 0° .*

These observations are specially interesting as giving additional proof that in the case of a metal of known purity the variation of resistivity, as the metal is continuously cooled, is such as to indicate that it would in all probability vanish at the absolute zero of temperature. In the case of mercury, we are able to obtain a metal in a state of almost perfect chemical purity, and which, when continuously cooled, passes into the solid condition under circumstances which are entirely favourable to the prevention of stresses in the interior of the metal, due to cooling. These measurements, therefore, afford a further confirmation of the law which we have enunciated as a deduction from experimental observations, that the electrical resistivity of a pure metal vanishes at the absolute zero of temperature.

PRSL 60

“On the Magnetic Permeability and Hysteresis of Iron at Low Temperatures.” By J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London, and JAMES DEWAR, LL.D., F.R.S., Fullerman Professor of Chemistry in the Royal Institution, &c. Received May 27,—Read June 11, 1896.

Although considerable attention has been paid to the changes produced in the magnetic properties of iron, particularly its magnetic permeability and hysteresis, at ordinary and at higher temperatures, but little information has been obtained up to the present on the behaviour of iron and steel as regards magnetic properties when cooled to very low temperatures. By the employment of large quantities of liquid air we have been able to conduct a long series of experiments on this subject, the results of which we propose here briefly to summarise, leaving for a future communication fuller details and discussion of the results. The experimental work has consisted in making measurements, chiefly by ballistic galvanometer methods, of the permeability and hysteresis loss in certain samples of iron and steel, taken in the form of rings or cylinders. The first experiments were concerned with the variation of the magnetic permeability of soft iron under varying magnetic forces, the iron being kept at a constant low temperature, obtained by placing it in liquid air in a state of very quiet ebullition in a vacuum vessel.

* This is in close agreement with the values obtained by Guillaume, Mascart, and Strecker for temperatures between 0°C. and $+30^{\circ}\text{C.}$

Experiments on Annealed Swedish Iron.

A cylinder of iron was formed by winding up a sheet of Sankey's best transformer iron (Swedish).* The width of the strip was 4.895 cm., the thickness 0.0356 cm.; three complete layers of the sheet iron were used in forming the core. The area of cross-section of the side of the cylinder so formed was 0.5229 sq. cm. The mean diameter of the cylinder was 3.612 cm. This cylinder of iron was placed in a clay crucible packed with magnesia, the lid luted on with fire-clay, and the crucible then raised to a bright red heat in a forge, after which it was allowed to cool very slowly. The iron cylinder was thus carefully annealed out of contact with air or any material containing carbon. This soft annealed iron ring was then wound over with silk ribbon, and two windings of silk-covered copper wire placed upon it; the first or primary circuit consisted of 131 turns of No. 26 double silk-covered wire; the secondary circuit consisted of 112 turns of No. 36 silk-covered copper wire. The magnetising force to which the ring is subjected when a current is sent through the primary coil is measured by the value of $4\pi/10 \times$ the ampère-turns per unit of length of the mean perimeter of the ring, and this, in the case of the present ring, reduces to the number 14.507 times the ampère current. The magnetising force in absolute units is therefore very closely given by the number obtained by multiplying the current flowing through the primary coil in ampères by 14.5. The resistance of the primary coil at about 15° C. was 0.92 ohm, and the resistance of the secondary at the same temperature 8.98 ohms. The secondary circuit of this ring coil or transformer was then connected through appropriate resistances with a ballistic galvanometer, having a resistance of 18 ohms. The primary circuit was connected through suitable resistances and a current reverser with a circuit of constant potential. By these arrangements it was possible to reverse a definite current passing through the primary coils, and by observing the throw produced by the ballistic galvanometer, to calculate the induction in the iron core. The galvanometer was calibrated by reversing a known current passing through a long solenoid, in the centre of which was placed a secondary coil of known turns and dimensions, which was always kept in series with the secondary coil of the transformer. In this manner a series of observations was taken with gradually increasing magnetising forces. Before commencing each series of observations, the ring was carefully demagnetised by passing through the primary coil an alternating current, which was gradually reduced in strength to zero, the ring coil being thus brought into a magnetically neutral condition. An increasing

* This sheet iron was kindly given to us by Mr. R. Jenkins, to whom our thanks are due.

series of primary currents was successively passed through the primary coil and reversed, the throw of the ballistic galvanometer being noted in each case. In the first set of observations the ring was kept at the ordinary temperature of the air, 15° C., and in the second set it was immersed in liquid air, and the following table shows the results, both for the high and for the low temperature observations.

After taking a complete magnetisation curve at the ordinary temperature, the ring was immersed in liquid air, bringing its temperature down to about -185° C., and a complete series of observations taken again in the same manner, previously having first carefully

Table I.—Magnetisation Curve of Annealed Soft Iron (Sankey's Transformer Iron).

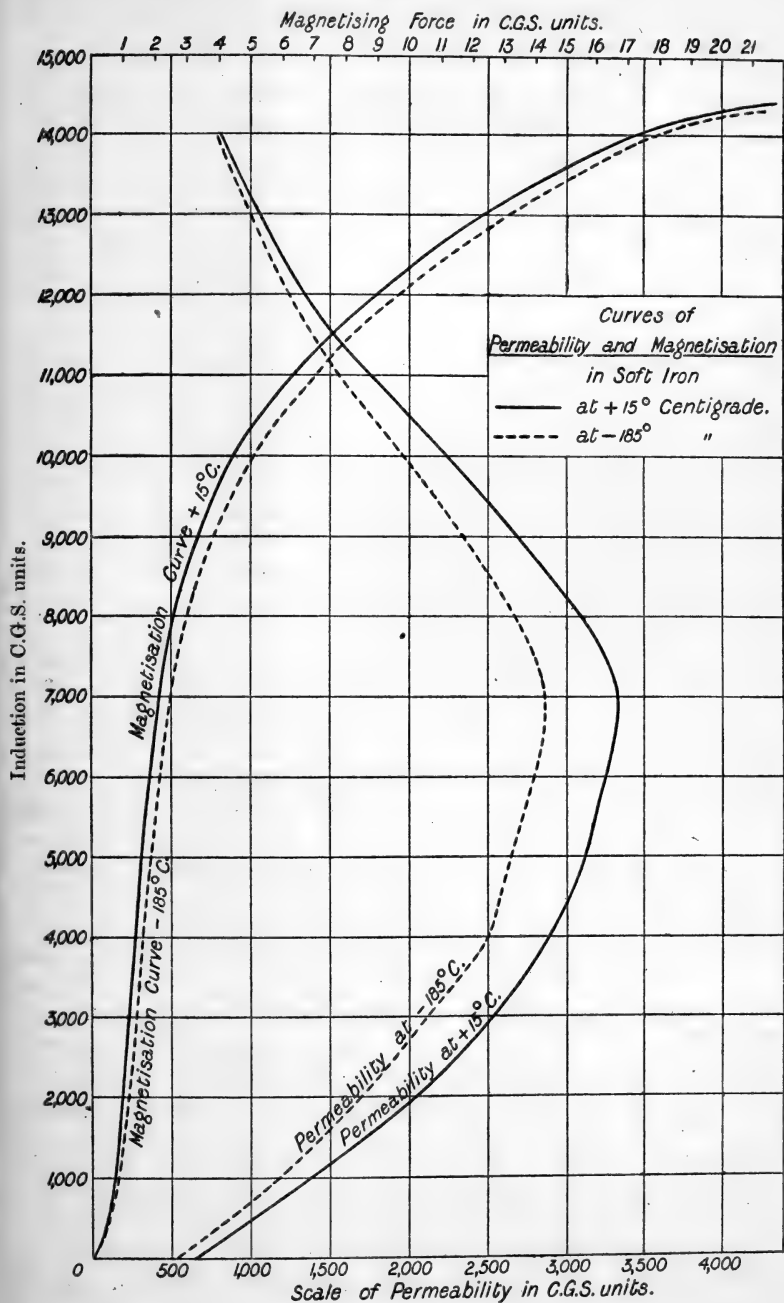
At 15° C.			At -186° C. (in liquid air).		
Magnetising force. H.	Induction. B.	Permeability. μ .	Magnetising force. H.	Induction. B.	Permeability. μ .
0·725	1000	1379	0·841	1000	1189
0·971	2000	2060	1·174	2000	1704
1·174	3000	2555	1·407	3000	2132
1·378	4000	2903	1·595	4000	2508
1·595	5000	3135	1·886	5000	2651
1·840	6000	3261	2·145	6000	2797
2·10	7000	3333	2·440	7000	2869
2·58	8000	3101	2·99	8000	2675
3·35	9000	2687	3·83	9000	2350
4·47	10000	2237	5·08	10000	1968
6·27	11000	1754	7·05	11000	1560
8·99	12000	1335	9·72	12000	1234
12·35	13000	1053	13·11	13000	992
17·22	14000	813	17·90	14000	782
22·1	14400	652	21·35	14300	670

demagnetised the ring as described by an alternating current. The ring was then taken out of the liquid air, allowed to warm up again to the ordinary temperature, and another complete set of observations taken at the ordinary temperature. In this manner a series of eighteen complete sets of observations were taken, about half of them being at 15° C. and half of them at -185° C. In cooling the ring in liquid air, it was found to be important to cool it slowly by holding it some time in the dense gaseous air lying over the liquid air. If suddenly plunged into liquid air the iron becomes hardened. It was found that after the first five sets of observations, which were some-

what variable, the annealed iron ring was brought into a completely stable condition, in which the curve of magnetic induction plotted in terms of magnetising force taken at the low temperature was different from that taken at 15° C. by a perfectly constant amount, the observations at the low temperature always lying on one curve, and those at the higher temperature always lying closely on the other curve. In the diagram in fig. 1 the two magnetisation curves are shown, the firm line curve being the magnetisation curve at 15° C., and the dotted curve being the magnetisation curve taken at -185° C. in the liquid air. The figures in Table I are the mean values obtained from the curves plotted from the thirteen sets of closely consistent observations. These curves show that the permeability of soft annealed iron is reduced when it is cooled to about 200° below zero, for the whole range of magnetic forces between zero and 25 C.G.S. units. The permeability curves for the two states are likewise similarly shown on the same chart. The maximum permeability for this iron corresponds with a magnetising force of about 2 C.G.S. units; the maximum permeability at the ordinary temperatures for this iron is 3400, being reduced to 2700 when the iron is cooled to the temperature of liquid air. The percentage reduction in permeability becomes less as the magnetising force is increased beyond or reduced below this critical magnetising force. These experiments were repeated, as above stated, many times very carefully with this ring of annealed soft Swedish iron, and also with a second ring of the same kind, and have invariably shown the same results, viz., that the permeability of soft annealed iron is decreased by being cooled to this low temperature within the range of magnetising forces from 0 to 25. It will be seen that the highest induction reached in the case of this iron is 14,500 C.G.S. units, corresponding to a magnetising force of 25. This iron is of very high magnetic quality, and is of the same character as that which is much used for the construction of alternating current transformers in commercial use.

A series of experiments was then made with the same transformer, keeping the magnetising forces constant, but allowing the iron to rise gradually in temperature up from the temperature of liquid air to 15° C. In these experiments the transformer was embedded in a mass of paraffin wax with a platinum wire resistance thermometer also embedded in the same mass in close contact with the ring coil. The paraffin wax encasing the ring coil and thermometer having been cooled down to the temperature of liquid air by immersing it in a large bath of the liquid air, it was then lifted out and placed in a vacuum-jacketed test-tube, so as to heat up with extreme slowness, and a series of observations taken by reversing a constant magnetising force at intervals, and observing at the same instant the temperature of the ring coil as given by the platinum thermometer.

FIG. 1.



The results of these observations are given in Table II, and these observations are set out in the curve marked soft annealed iron in fig. 2.

Table II.—Variation of the Magnetic Permeability of Soft Annealed Swedish Iron with Temperature.

Magnetising force = 1.77 C.G.S.

Temperature measured in platinum degrees by standard thermometer P₁.

Temperature.	Permeability.
0°	2835
— 20	2815
— 40	2770
— 60	2727
— 80	2675
— 100	2622
— 120	2560
— 140	2497
— 160	2438
— 180	2381
— 200	2332

The results show that as the temperature rises up from -185° C., or -200° on the platinum scale temperature, up to the ordinary temperature, the permeability of the soft iron for the particular magnetising force selected increases perfectly uniformly, the curve of increasing permeability with temperature being nearly a straight line.

In the next place, we have examined the hysteresis of the same soft iron ring at different temperatures and for different maximum inductions. These observations were carried out by taking a complete series of hysteresis curves with the ballistic galvanometer, gradually increasing the inductions from zero to 12,000. After the complete hysteresis curves were obtained, their areas were carefully integrated with an Amsler planimeter, and the values reduced so as to express the hysteresis loss in watts per lb. per 100 cycles per second, and these values plotted in terms of the maximum value of the magnetic induction per square centimetre of the iron core corresponding to each particular hysteresis loss. Nothing would be gained by giving the full details of all the observations by which these hysteresis curves were obtained. They were exceedingly numerous, and the tedious nature of the ballistic observations made it a matter of prolonged observation to secure the whole series necessary, but the final results are shown in Table III. The curve in fig. 3 represents the increase of hysteresis loss with induction, and the observations which

FIG. 2.

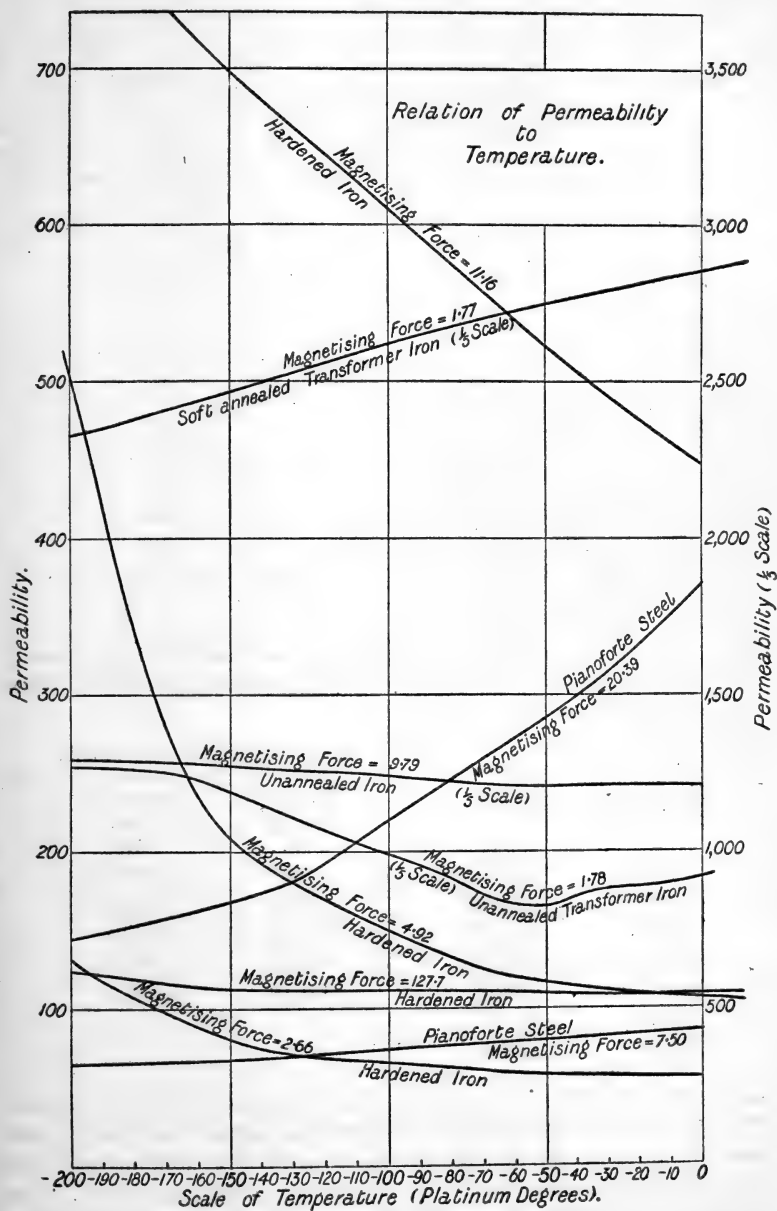


FIG. 3.

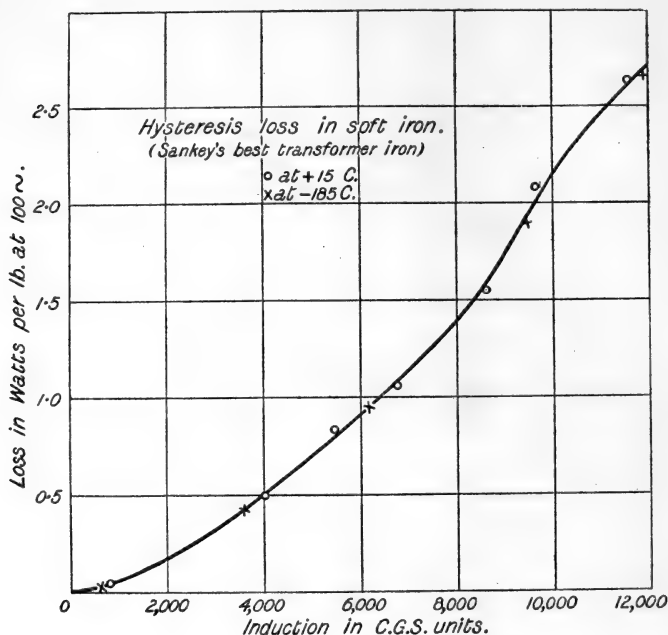


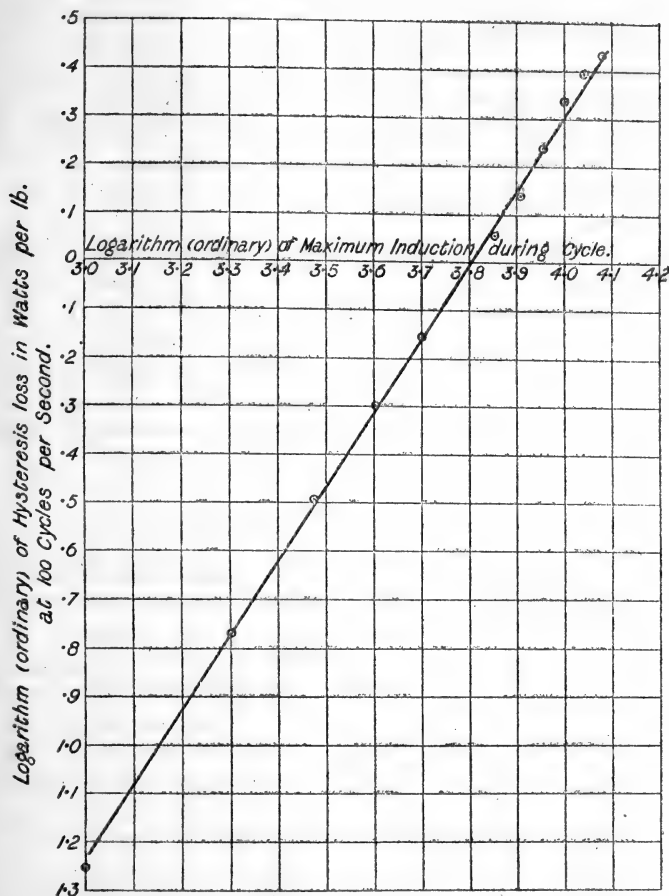
Table III.—Hysteresis Loss in Soft Annealed Swedish Iron in Watts per pound per 100 cycles per second for various maximum Inductions.

I. At +15° C.		II. At -185° C. (in liquid air).	
Maximum induction.	Hysteresis loss.	Maximum induction.	Hysteresis loss.
B.	W.	B.	W.
844	0.0397	688	0.02519
4026	0.4957	3603	0.4246
6743	1.062	6185	0.949
9687	2.070	9461	1.907
11618	2.632	11916	2.658
8593	1.545		
5516	0.823		

were taken at ordinary temperatures are denoted by the small circles. The observations for hysteresis loss which were taken at the temperature of liquid air are denoted by the crosses. It will be seen that substantially the circles and the crosses lie on the same curve. The results of these observations, therefore, show that there is practically no change in the hysteresis loss in soft iron by cooling it to the tem-

perature of liquid air. If, instead of plotting the hysteresis loss and induction, the ordinary logarithms of these quantities are taken as coordinates, the curve, as shown in fig. 4, then obtained is

FIG. 4.



very nearly a straight line as far as the limit of an induction of about 9000, and from the inclination of this line it is clear that the hysteresis loss, W , in watts per lb. per 100 cycles is found to be related to the maximum induction B in C.G.S. units per square centimetre by the law $W = \frac{1156}{10^9} B^{1.56}$, or, if the hys-

teresis loss is reckoned in ergs per cubic centimetre per cycle = W' , then $W' = 0.002 B^{1.56}$. These results are quite in accordance

with certain conclusions of Messrs. Laws and Warren (see 'Proceedings of the American Academy of Sciences,' vol. 30, p. 490). These observers made a series of experiments on a material which was practically a very soft steel, and employing a differential watt meter, measured the hysteresis loss in the iron at varying and increasing temperatures up to 600° or 700° . They found that the hysteresis loss in this material did not begin to decrease sensibly until about 150° C.; after that it decreased regularly in accordance with the simple linear function of the temperature. In one experiment which they tried with the same material cooled to -78° C. in solid carbonic acid and ether, they found no difference between the hysteresis loss of this soft steel at that temperature and at the ordinary temperatures. Our results, which have been carried to the much lower temperature of liquid air, indicate that in the case of soft annealed Swedish iron the hysteresis loss is not changed by cooling from ordinary temperatures to the temperature of liquid air. As we know that the hysteresis loss of soft iron decreases when the temperature is increased, from the ordinary experience with transformers in commercial use, the matter that requires further investigation is to discover the temperature at which the hysteresis loss sensibly changes and begins to diminish.

Experiments on Unannealed Swedish Iron.

We have also carried out a series of experiments of the same character with unannealed iron and steel. A ring coil was constructed of sheet iron of the same quality as that forming the core of the soft iron transformer above described, but no special pains were taken to anneal the iron, and as it was "hardened" in a magnetic sense by being bent into shape, this difference in quality showed itself in the magnetic observations. A ring coil was constructed of the following dimensions:—The thickness of the strip was 0.031 cm., width of the strip 1.24 cm., the ring was formed by $23\frac{1}{2}$ layers of this sheet iron wound up closely into the form of a ring. The outside diameter of this ring was 4 cm., the inside diameter 2.13 cm., the cross-section of the iron in the ring was therefore 0.9032 sq. cm., and the mean perimeter of the ring 9.62 cm. This iron ring was not annealed in any way, but it was simply wound over with silk ribbon, and then had placed upon it two coils of wire. The primary coil consisted of 150 turns of No. 26 wire, having a resistance of 0.383 ohm, and the secondary coil consisted of 240 turns of No. 36 wire having a resistance of 8.092 ohms. As the diameter of cross-section of the ring was not very small compared with the mean diameter of the ring, it was necessary to calculate by a proper integration the mean value of the mean magnetising force in terms

of the current passing through the primary coil, and it was found that the mean magnetising force to which the iron was exposed was closely expressed by the value 20·219, multiplied by the ampère current flowing through the primary coil. This coil had its secondary circuit connected up to the galvanometer, as above described, and a series of observations were taken with this coil by reversing a constant magnetising current passing through the primary coil, and observing the throw of the ballistic galvanometer connected with the secondary circuit. The ring coil, together with the platinum thermometer, was embedded, as above described, in a mass of paraffin wax, and the whole mass, after having been cooled down to the temperature of liquid air, was slowly allowed to heat up again. Observations were taken with two different magnetising forces over the range of temperature from -185° C. up to the ordinary temperature, and from the calculated induction in the ring determined for each magnetising force, the permeability was found corresponding to each particular force and temperature. The results of these observations are given in Table IV, and are delineated in fig. 2, in the form of two curves marked unannealed iron.

Table IV.—Variation of Magnetic Permeability of Unannealed Swedish Iron with Temperature.

Temperature measured in platinum degrees by standard thermometer P_1 .

Temperature.	Permeability.	
	Magnetising force, 1·78.	Magnetising force, 9·79.
0°	917	1210
— 20	885	1212
— 40	857	1212
— 60	832	1208
— 80	913	1230
— 100	993	1240
— 120	1067	1255
— 140	1153	1265
— 160	1230	1280
— 180	1262	1290
— 200	1272	1293

The results of the observations, as indicated in fig. 2 in the curves marked Unannealed Iron, show that for this unannealed iron the permeability increases as the temperature falls, and is exactly the reverse in the case of the same quality of iron carefully annealed. The difference, also, between the two materials is very marked

at low temperatures. The soft annealed iron if cooled slowly to -185° C. recovers its original permeability when heated up again to ordinary temperatures. The unannealed iron, however, after cooling to the same low temperature, retains some of its increased permeability when heated up again to 15° C. The unannealed iron cannot be taken over the temperature range again and again with the same definite permeability values at each recurrent temperature, as in the case of the soft annealed iron. The unannealed iron is more or less permanently changed in magnetic character every time it is heated or cooled.

With this transformer, a long series of observations were taken to determine the hysteresis loss corresponding to different inductions when taken at the ordinary temperatures, and the temperature of liquid air. The hysteresis cycles were taken with the ballistic galvanometer over wide ranges of maximum induction, the transformer being alternately at the ordinary temperature and in liquid air, but no constant magnetic condition could be obtained. In one set of observations, at a given maximum induction the hysteresis loss was increased when the transformer was raised in temperature, and for another series of observations at the same induction it was diminished. It is therefore impossible to make any definite statement with regard to the magnetic hysteresis loss in this unannealed iron ring coil at the two temperatures. The mere fact of immersing the unannealed iron in the liquid air changes its magnetic qualities to such a degree that it is no longer the same material, magnetically considered, after, as before its immersion. One curious fact, however, was noticed very soon with regard to unannealed iron, and that is, that if the unannealed iron ring coil has a small magnetising current passed through its primary coil, the secondary coil being connected to the galvanometer, the sudden immersion of this ring coil into liquid air invariably causes a deflection of the ballistic galvanometer, even when the primary magnetising current remains perfectly constant in value, thus showing a sudden and very large increase in the permeability of the unannealed iron. Whilst the iron is in the liquid air it retains this increased permeability. If brought suddenly out its permeability again diminishes, but not with equal rapidity. This is partly accounted for by the fact that the iron is cooled with immense rapidity when it goes into the liquid air, but it heats up again much more slowly when it is brought out. The definite fact, however, remains, which has been repeatedly observed, that the cooling of this unannealed iron to a low temperature always increases its permeability, as far as we know, no matter whatever may be the magnetising force employed. One difficulty experienced in dealing with unannealed iron is the fact that in taking it up to the high magnetising forces, and by the process required to remove residual

magnetism by the application of an alternating current, the iron is so altered in magnetic qualities that it is impossible to repeat two sets of observations under precisely similar circumstances. With regard to the unannealed iron, it may be noted that if an ordinary magnetisation curve is taken up to very high magnetisation forces, and the iron then demagnetised by the application of an alternating current gradually reduced, the first magnetisation curve can never be repeated exactly again on applying increasing magnetisation forces, but a curve is obtained which lies slightly inside the first curve, and which indicates that the permeability has been reduced. The subsequent repetition of this process will give a series of curves which occupy different positions, but which do not precisely repeat any of them. Hence it is impossible to repeat at a constant temperature with this unannealed iron exactly any magnetisation or permeability curve. In the case of the annealed iron it is quite different. A magnetisation curve can be obtained after having carefully de-magnetised the iron, if this magnetisation is pressed up to nearly its limit and the iron then de-magnetised by the application of an alternating and decaying magnetising force, a second magnetisation curve can be obtained on again applying an ascending magnetising force, but it will not coincide exactly with the first curve. The annealed iron can, however, be brought back into its original condition by dipping it a few times into liquid air. Under these conditions, we have been able to repeat as frequently as required the observations with the annealed iron taken at the different temperatures. In the case of the unannealed iron the changes produced in it by immersing it in the liquid air and by magnetising and demagnetising it, are such as to render it almost impossible to obtain results capable of precise repetition, with respect to the hysteresis loss and permeability for varying magnetising forces.

Experiments with Hardened Iron.

A third set of experiments were taken with a ring coil of the same dimensions as the ring coil made of soft annealed transformer iron first described. This third coil was constructed of the same sample of Sankey's transformer sheet iron as the above described soft annealed ring, but it was treated subsequently to its formation in the following manner :—

A short piece of iron gas-pipe was made red hot in a forge; the ring coil, having been constructed, was dropped into the red-hot pipe, and the ends of this pipe loosely plugged up with slag wool; the red-hot pipe was then covered over with cinders, and the mass allowed to cool. Under these conditions the ring coil was annealed in an atmosphere of carbonic oxide and in contact with hot carbon; the sheet

iron was, therefore, under these circumstances, case-hardened, and will be referred to as the hardened iron ring. Having been formed into a transformer in the above-described manner, a long series of observations were taken with this coil to determine its permeability at different temperatures and with different magnetising forces. The results of these observations are shown in the Table V below, and are delineated graphically in the curves in fig. 2, marked Hardened Iron. The results show in a remarkable manner that the iron so treated undergoes a very considerable increase in magnetic permeability when it is cooled to the temperature of liquid air; for certain magnetising forces the permeability at the lowest temperature reached may be increased as much as five times. In this respect, therefore, this iron presents in an exaggerated degree the same qualities found in the unannealed iron.

Table V.—Variation of Magnetic Permeability with Temperature of Hardened Iron.

Temperature measured in platinum degrees by standard thermometer P_1 .

Temperature.	Permeability.			
	H = 2·66.	H = 4·92.	H = 11·16.	H = 127·7.
0°	56·0	106·5	447·5	109·0
– 20	57·0	109·5	476·0	108·5
– 40	58·0	114·0	506·5	109·0
– 60	59·0	119·8	540·0	110·5
– 80	62·5	132·5	575·0	111·0
– 100	65·5	150·0	610·0	112·0
– 120	69·2	169·3	645·0	112·0
– 140	75·3	192·5	680·0	112·3
– 160	89·5	236·0	717·0	114·0
– 180	107·5	338·0	762·0	119·5
– 200	132·0	502·0	823·0	124·0

Experiments with Steel.

We have also examined the behaviour of a ring coil made of steel pianoforte wire. We have found in this case the curious result that pianoforte steel behaves in the same manner as the annealed soft iron; its permeability is decreased as the temperature is lowered. The results of the measurements with this steel-core ring are shown in Table VI, and graphically in the curves in fig. 2, marked steel.

Table VI.—Variation of Permeability with Temperature.

Pianoforte Steel.

Temperature measured in Platinum degrees by standard thermometer P_1 .

Temperature.	Permeability.	
	Magnetising force, 7.50.	Magnetising force, 20.39.
— 0°	86.0	361.0
— 20	84.0	332.5
— 40	81.0	299.5
— 60	79.0	271.5
— 80	77.0	246.5
— 100	74.0	220.0
— 120	71.5	193.0
— 140	68.5	174.3
— 160	67.0	163.0
— 180	66.0	153.0
— 200	64.5	144.0

We propose to continue the examination of the anomalous behaviour so presented by iron in different states of hardening by examining in the same way the changes of permeability in the case of several iron rings of the same dimensions formed in the one case of soft annealed iron, and in another case of the same quality of iron hardened, and in the remaining cases using steel of known composition at different states of temper. We desire to add that in the conduct of this research we have been under great obligations to Mr. J. E. Petavel for rendering us very efficient assistance in taking the exceedingly tedious ballistic galvanometer observations, and in reducing them when taken.

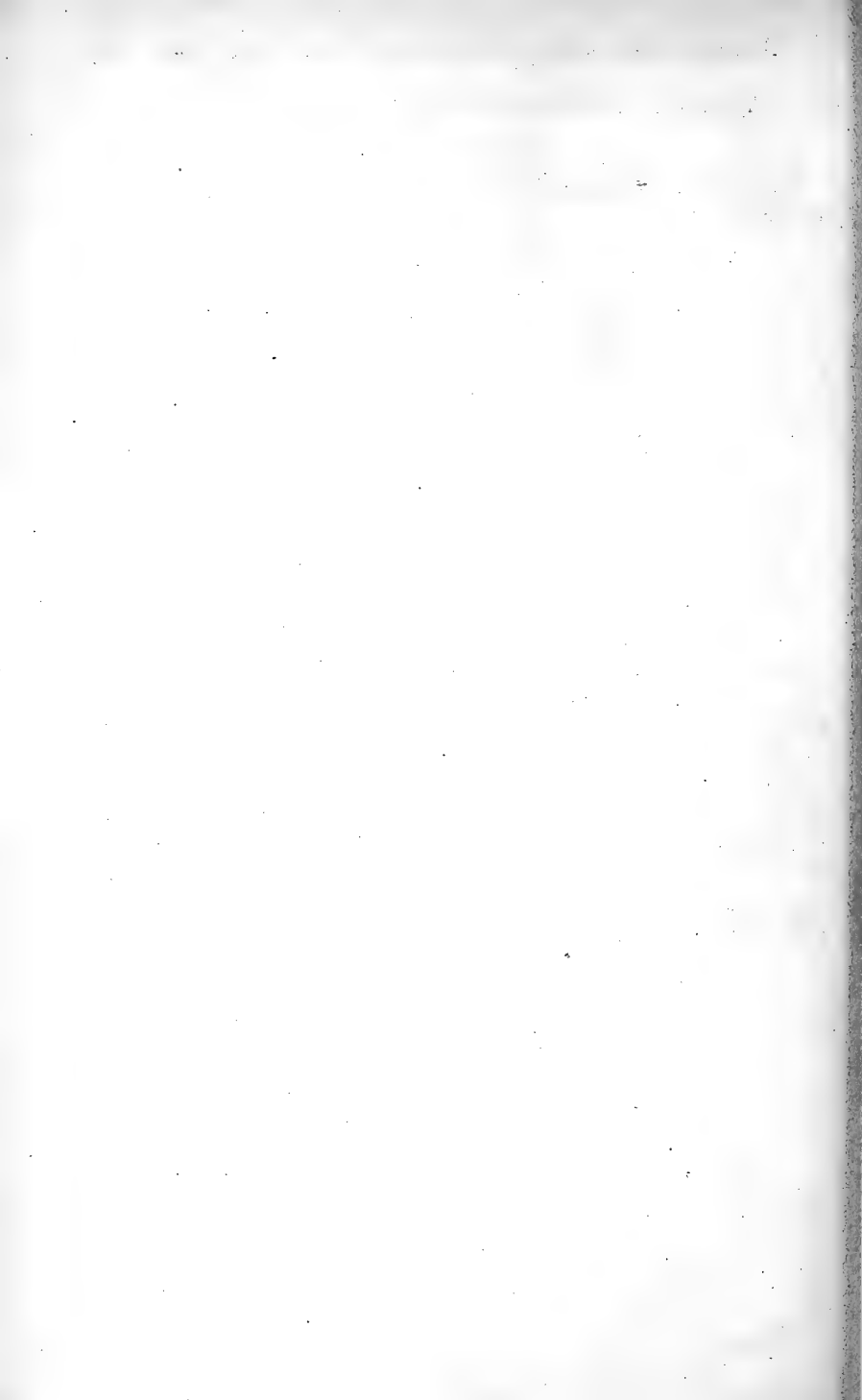


Table VI.—Variation of Permeability with Temperature.

Pianoforte Steel.

Temperature measured in Platinum degrees by standard thermometer P₁.

Temperature.	Permeability.	
	Magnetising force, 7·50.	Magnetising force, 20·39.
— 0°	86·0	361·0
— 20	84·0	332·5
— 40	81·0	299·5
— 60	79·0	271·5
— 80	77·0	246·5
—100	74·0	220·0
—120	71·5	193·0
—140	68·5	174·3
—160	67·0	163·0
—180	66·0	153·0
—200	64·5	144·0

We propose to continue the examination of the anomalous behaviour so presented by iron in different states of hardening by examining in the same way the changes of permeability in the case of several iron rings of the same dimensions formed in the one case of soft annealed iron, and in another case of the same quality of iron hardened, and in the remaining cases using steel of known composition at different states of temper. We desire to add that in the conduct of this research we have been under great obligations to Mr. J. E. Petavel for rendering us very efficient assistance in taking the exceedingly tedious ballistic galvanometer observations, and in reducing them when taken.

“Observations on Atmospheric Electricity at the Kew Observatory.” By C. CHREE, Sc.D., Superintendent. Communicated by Professor G. CAREY FOSTER, F.R.S. Received May 11,—Read June 4, 1896.

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PART I.

The Measurement of Potential in Theory and Practice.

§ 1. An electrograph belonging to the Meteorological Office has been in operation at Kew Observatory, with interruptions, since 1861. The results obtained in the early years of its existence were dis-

cussed in 1868 by Professor Everett,* and the results obtained in 1880 were discussed in 1881 by my predecessor, Mr. Whipple.† Nearly two years ago, with the approval of the Kew Observatory Committee and the Meteorological Office, I commenced an investigation intended as preliminary to a consideration of the expediency of further publication of the electrograph records.

My first object was to find out whether definite quantitative measurements of potential could be derived from the electrograph curves. To aid in this investigation observations have been made at several spots near the Observatory with a portable electrometer, by White, of Glasgow, whose scale value was determined at University College by the kind assistance of Professor Carey Foster.

To render intelligible the bearing of these observations on the question, a brief description is required of the nature and position of the electrograph.‡ It consists essentially of a water-dropper and a quadrant electrometer. The water is held in a can, some 14 inches high and 15 inches in diameter, supported on three insulators of the Mascart pattern. From the can a tapering tube, resting on a fourth insulator, projects through a hole in a window facing the west. The end of the tube whence the water issues is $4\frac{1}{2}$ feet from the west wall of the Observatory, and 10 feet above the ground. The stream of water is regulated by two taps in the long tube. From the water-dropper an insulated wire passes to the needle of the quadrant electrometer. One pair of quadrants are kept at a given positive potential, the other pair at an equal negative potential, by means of a battery of 60 cells in series whose centre is to earth. The needle suspension carries a mirror, and light reflected from it produces a curve on photographic paper which is wound round a cylinder driven by clock-work. The position of the base line answering to the earth's potential—treated as zero—is obtained by putting the electrometer needle to earth, twice at least for each curve. Of late years the value of the curve ordinates, in volts, has been obtained from time to time by connecting the electrometer needle and one terminal of the portable electrometer, and varying their joint potential by means of an electrophorus. Simultaneous readings are taken of the curve ordinate and the portable electrometer.

If the ideal were attainable, the stream from the water-dropper should break up exactly at the end of the tube, and be always sufficiently copious to ensure the immediate picking up by the can and the electrometer needle of the potential existing in the air at the spot in question.

* 'Phil. Trans.' for 1868, p. 347.

† 'B. A. Report,' vol. 51, p. 443.

‡ (July 28.) Some alterations have been effected since the above was written.

Interpretation of Electrograph Record.

§ 2. The first question is: supposing the apparatus perfect, does the electrograph supply information as to the potential anywhere except at the spot where the stream of water breaks into drops? To answer this question, one has to consider the influence of the environment, notably the proximity of a lofty building.

An investigation into this point was made ten years ago by Professor Exner, of Vienna, who found the equipotential surfaces near a building much deflected from horizontality. His results indicated apparently that for practical purposes the whole building might be regarded as possessing the earth's potential. Whilst it was anticipated that Exner's conclusions would hold good of Kew Observatory, it appeared prudent as a check to take observations with the portable electrometer, at a series of points in a vertical plane perpendicular to the west wall near the water-dropper. Observations were taken at heights of 3, 6, and 9 feet from the ground, which possesses, it may be explained, a slope away from the building. The base line, starting at the Observatory wall, terminated 57 feet away in a parallel wall 11 feet high, belonging to a much lower building. The observations were repeated on several days, but one example will suffice. The potential measurements are in volts, the distances from the Observatory wall in feet.

Table I.

Observations on November 6, 1894.

												Mean
Distance from wall.....	3	6	12	18	24	30	36	42	48	54		potential.
Potential at height 3 feet..	4	6	18	38	48	46	34	24	16	6		26
" 6 " ..	8	18	40	58	88	84	76	68	52	22		56
" 9 " ..	—	28	44	76	102	120	120	108	68	36		78

In forming the means in the last column the results at 3 feet from the Observatory wall were omitted. The readings were uncorrected for variations of potential during the interval occupied by the observations.

So far as they go, the results are clearly confirmatory of Exner's. They show that the influence of a tall building in pulling down the potential extends to a considerable distance.

§ 3. The large dependence of the electrograph records on the immediate environment of the water jet complicates matters, but this need not prove a serious obstacle if the conditions allow us to regard the problem as one of statical electricity, in which influencing bodies are either stationary or at a distance. On this hypothesis, simultaneous potentials at any two neighbouring points would stand to one

another in a practically constant ratio, a function only of their geometrical coordinates.

If once this ratio were determined, one could deduce the potential at either point from that observed at the other. Regarding the spot where the water jet breaks up as one of these points, and selecting for the other a spot sufficiently distant from the building, one could deduce the *potential gradient* in the open, *i.e.*, the increase in voltage per unit of height above the ground. This point of view was apparently acted upon by Exner,* and by Elster and Geitel.† In both instances the existence of corroborative evidence is referred to, but I am not aware that particulars have been published. It would also appear that Exner and Elster and Geitel directed their attention mainly, if not exclusively, to clear quiet days.

There being no limitation to the use of the Kew electrograph, it appeared advisable not to restrict the investigations to days of a special kind, or to a particular season of the year.

Selection of Stations.

§ 4. It appeared desirable to compare the potential at more than two stations, so as to ensure a sufficient variety in the surroundings. I shall distinguish the stations selected by the letters A, B, C, D, E, F. Of these A is the flat top of a stone pillar, $3\frac{3}{4}$ feet high, in the Observatory garden, about 56 yards from the Observatory; it is surrounded by a frequently mown grass lawn. B is the top of a temporary wooden stand, $6\frac{1}{2}$ feet high, and only $3\frac{1}{2}$ feet from the west wall of the Observatory. C is the centre of a flat plank supported $3\frac{3}{4}$ feet above the ridge of a wooden building, situated about 100 feet to the south-west of the Observatory; it is 18 feet above the ground. D is on the south side of a stone parapet, $2\frac{1}{8}$ feet high, encircling the flat roof of the Observatory; it is 37 feet from the ground. E is the top of a camera stand, $5\frac{1}{2}$ feet above the Observatory roof, and 17 feet to the east of the central dome. F is the top of a stand on the roof—used for testing anemometers—level with the cups of the standard anemometer, from which it is distant about 17 feet to the north; it is 57 feet above the ground.

The observations were taken with the portable electrometer, and, as the burning end of the fuse was at a height of some 12 to 16 inches above the base of the electrometer, an addition of, say, $1\frac{1}{4}$ feet requires to be made to the altitudes of the several stations to get the height from the ground of the spot whose potential was measured.

A was the only station that could be regarded as practically uninfluenced by the neighbourhood of a building, and even in its case we

* 'Wien. Sitz.,' vol. 98, 1889.

† 'Wien. Sitz.,' vol. 101, p. 703, 1892.

have the influence of a massive stone pillar some $2\frac{1}{2}$ square feet in section. A calculation of the potential gradient which regards the observations at A as referring to a spot 60 inches above the ground in the open is certain to give an under-estimate. As it is impossible, however, to dispense with a support of some kind, and the presence of the observer is also a disturbing influence, no exact allowance can be made for this.

There have been four principal series of observations. In the first, occupying part of November and December, 1894, observations were taken, when practicable, once a day at stations A, B, C, D, and latterly at E also. In the second series, during part of March and April, 1895, observations were usually taken about 10.30 A.M. and 4.30 P.M. at each of the stations except F. The third series, during part of June and July, 1895, closely resembled the second; and the only material difference in the fourth was the substitution of station F for station D.

No observations were taken on Sundays or on Saturday afternoons. The observations were taken in a fixed order, and, thanks to the skill of the observer, Mr. E. G. Constable, a complete set of readings occupied only some seven or eight minutes. The time scale of the electrograph curves is far from open, and for this and other reasons I have judged it best not to attempt to reduce the readings with the portable electrometer to a common instant.

Comparison of Results at the different Stations.

§ 5. I have taken A as base station, and have found the ratios borne to the individual readings there by the corresponding readings at the other stations.

Let r_A , r_B represent corresponding readings at A and B, and let

$$r_{B/A} = \frac{1}{n} \Sigma (r_B/r_A),$$

where Σ denotes summation for a series of n observations. Then $r_{B/A}$ may be called the *mean value of the ratio* for the series of observations.

Also let us apply the term *percentage deviation of the ratio from its mean* to the quantity

$$\frac{\Sigma \{ (r_B/r_A) - r_{B/A} \}}{nr_{B/A}} \times 100,$$

in which the terms in the numerator are taken irrespective of sign.

Table II gives the extreme and mean values of the ratios during each series of observations, excluding three or four occasions when negative potentials were met with.

Table II.

Series of observations.	Number of observations.	r_B/r_A .			r_C/r_A .			r_D/r_A .		
		Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.
I.	25	0·38	0·17	0·26	3·05	1·11	2·22	3·33	1·46	2·41
II.	45	0·54	0·16	0·29	2·32	1·40	1·78	4·52	1·46	2·28
III.	31	0·50	0·17	0·27	2·29	1·00	1·70	3·67	1·11	2·14
IV.	23	0·41	0·09	0·22	2·86	1·33	1·92	—	—	—

Series of observations.	Number of observations.	r_E/r_A .			r_F/r_A .		
		Max.	Min.	Mean.	Max.	Min.	Mean.
I.	25	4·95	2·46	3·12	—	—	—
II.	45	6·30	1·74	2·68	—	—	—
III.	31	4·33	1·11	2·51	—	—	—
IV.	23	4·73	2·05	2·87	8·34	2·71	4·53

In series I there were only twelve observations at station E. In series III the mean ratios for the higher stations are depressed by one abnormally low reading. The means in the different series vary, but the differences are too small to warrant any positive conclusion. They indeed suggest the possibility of the potentials at the higher stations being relatively somewhat higher in winter than in summer, but this may arise from a slight want of uniformity in the procedure followed at the different seasons.

The departures of the maxima and minima in Table II from the means are considerable, but the number of instances in which the departures from the mean are large is in reality small. This will be seen by reference to Table III, which gives the percentage deviations of the ratios from their means, treating each series of observations separately.

Table III.

Percentage Deviations from the Means.

Series of observations.	r_B/r_A .	r_C/r_A .	r_D/r_A .	r_E/r_A .	r_F/r_A .
I.	14	14	15	15	—
II.	19	10	19	21	—
III.	19	11	20	20	—
IV.	28	13	—	16	20

The irregularity in r_B/r_A may be due in part to the slightly unsteady character of the stand forming station B. The potentials at B were also much the lowest, so that errors of reading were there of most importance. At the highest station, F, the variations occurring in the potential sometimes made accurate measurements difficult.

§ 6. To give a clearer idea of the degree of uniformity shown by Table III, I give in Table IV the extreme and mean readings at the several stations, omitting, as in Table II, occasions of negative potential.

Table IV.

Readings in Volts at the several Stations.

Series of observations.	A.			B.			C.		
	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.
I.	264	104	158	66	22	40	552	120	352
II.	708	50	206	215	15	58	1320	81	364
III.	174	27	100	45	6	27	306	27	171
IV.	830	29	249	115	12	50	1452	52	476

Series of observations.	D.			E.			F.		
	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.
I.	648	152	385	776	264	524	—	—	—
II.	1464	122	455	1688	182	531	—	—	—
III.	354	30	210	498	30	246	—	—	—
IV.	—	—	—	1785	93	662	2362	122	1032

On one exceptional day the potential at A varied from -1200 to $+1290$ volts in less than forty minutes; at station F it varied from -2424 to over $+4000$ volts in about the same time.

Constancy of Ratios during the Day.

§ 7. Table V gives the mean values of the ratios for the forenoon and afternoon observations, treated separately, during those days when there were readings at both 10.30 A.M. and 4.30 P.M. The days available numbered 17, 10, and 9 respectively in the second, third, and fourth series of observations. The headings "A.M." and "P.M." distinguish the forenoon and afternoon observations.

In each case treated in table V the mean value of the potential for the forenoon was considerably higher than that for the afternoon. Thus, at station A the ratio of the forenoon to the afternoon mean potential—for those days only on which there were both forenoon

Table V.

Forenoon and Afternoon Ratios.

Series of observations.	$r_{B/A}$		$r_{C/A}$		$r_{D/A}$		$r_{E/A}$		$r_{F/A}$	
	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.
II.	0·29	0·28	1·82	1·83	2·24	2·21	2·62	2·57	—	—
III.	0·24	0·33	1·75	1·52	2·16	1·87	2·57	2·17	—	—
IV.	0·22	0·23	1·96	1·88	—	—	2·76	2·87	3·99	4·46

and afternoon observations—was 1·37 in series II, 1·23 in series III, and 1·48 in series IV.

The difference between the mean potentials at the two hours on the specified days being so large, we may reasonably suppose that if any two other hours had been selected results would have been obtained showing a degree of accordance similar to that in Table V. The degree of accordance in the case of series II is truly remarkable, and in series IV, considering the smaller number of observations, it is but little inferior. If series III stood alone, we might suspect that in the afternoon the potential fell off more at the higher stations than at the lower, and this *may* of course be a true phenomenon of the season, midsummer, to which that series belongs.

Possible Dependence of Ratios on the Weather.

§ 8. It is conceivable that under one regular set of climatic conditions the potentials at the higher stations might relatively to the lower be either abnormally high or abnormally low. To test this point, the observations in each series have been divided into sets, according to the value of such a ratio as r_E/r_A . Attention has been confined to series II, III, and IV, as in series I the times of observation were less regular; but the forenoon and afternoon observations in series II and III have been considered separately.

Supposing the number of measures of, say, r_E/r_A available in any one instance to be $2n$ or $2n+1$, the n cases in which the ratio is largest form one set, the n cases in which it is smallest the other. For each of these sets the corresponding mean values of certain meteorological elements have been calculated, the data for the individual times of observation being derived from the self-recording instruments employed in the Observatory. The figures as to aqueous vapour and humidity have been deduced from the thermograms, with the aid of a modification of Glaisher's table, compiled by the Meteorological Office.

By "sunshine in hours" is meant the number of hours of sunshine measured by the Campbell-Stokes recorder up to the time of observation. The data under this head have been limited to the most sunny series of observations, viz., II and III.

The results are exhibited in Table VI, which shows also the maxima and minima values of the meteorological elements observed during the several sets of n observations.

There is in Table VI no uniform and conspicuous connexion between the value of $r_{E/A}$, or $r_{F/A}$, and the corresponding value of any one of the meteorological elements considered. In the case alike of barometric pressure and temperature the second mean—answering to the n lowest values of $r_{E/A}$ or $r_{F/A}$ —is higher than the first in five instances out of six. The differences between the two means are generally, however, so small that the phenomenon may be purely accidental. In the afternoon observations of series II there is a somewhat conspicuous association of a low value in $r_{E/A}$ with a high value of previous sunshine; but in series III there is no trace of such a phenomenon.

The question whether there may not be certain *occasional* types of weather, whose influence is masked in such a table as VI, which are associated with either a high or a low value of the ratio $r_{E/A}$, remains, I think, open. Evidence is in my hands which leads me to believe that during a low ground fog the potential gradient as a rule is decidedly higher near the ground where the fog is thick than higher up where the fog is slight.

Summary of Results at Different Stations.

§ 9. The conclusion I am disposed to draw, though I regard it as only a probability, is that such general phenomena as diurnal or annual variation of potential near the ground in the open may be deduced with fair accuracy by applying a constant factor to the records of a portable electrometer, employed regularly at a fixed point on the Observatory roof or near its walls. It must be remembered, however, that all six stations were comparatively close together, and that the equipotential surface passing through the highest station would be in the open perhaps only 14 or 15 feet above the ground. There is thus no evidence to warrant the deduction of conclusions for a spot a mile or two away or a few hundred feet above the ground.

On the trustworthiness of individual results deduced by means of a constant factor, one would not, after inspecting Tables II and III, be disposed to place much reliance. This question can hardly, however, be settled satisfactorily unless one have apparatus for taking the observations at the different stations absolutely simultaneously. The largest departures from the means in Tables II and III are

Table VI.

Observations.		Ratio treated.	Value of <i>n</i> .	Mean value of ratio.		Aqueous vapour, grams per cubic metre.	Barometer in inches.			Temperature, Fahrenheit.			Wind velocity, miles per hour.			Sunshine in hours.		
Series.	Time.			<i>n</i> largest values.	<i>n</i> smallest values.		Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.
II.	Forenoon	<i>r</i> _{E/A} .	12 {	3·09	—	9·03 4·67 6·70 8·54 5·25 6·66	30·20 28·86 29·72 30·37 29·27 29·97	53·1 41·6 47·3 56·4 39·1 47·6	40 4 14 21 0 8	3·4 0 1·0 4·2 0 1·1								
"	Afternoon	"	10 {	3·42	—	9·19 4·69 6·74 8·95 5·22 6·50	30·17 28·92 29·64 30·35 29·08 29·89	61·6 43·0 49·9 61·0 43·2 52·6	27 1 12½ 23 4 15	9·4 0 1·9 10·2 0·7 5·2								
III.	Forenoon	"	8 {	3·09	—	12·13 8·36 9·68 12·62 6·33 9·28	30·41 29·68 30·05 30·38 29·56 29·99	72·9 57·5 67·7 68·8 63·0 65·5	13 5 9 24 3 10	5·5 0 2·7 5·7 0 2·6								
"	Afternoon	"	7 {	2·61	—	10·81 8·68 9·87 13·26 6·90 9·95	30·27 29·62 29·88 30·40 29·69 30·07	79·1 65·1 70·9 76·2 67·2 71·7	22 4 14 27 4 13	11·6 1·2 6·9 11·1 1·3 6·3								
IV.	Forenoon and afternoon	" }	11 {	3·35	—	7·76 4·41 5·54 9·07 4·02 6·81	30·52 29·48 29·95 30·47 29·57 30·07	49·7 35·4 42·1 57·0 34·2 46·3	18 1 8 29 0 9	— — — — — —								
"	"		11 {	5·46	—	9·07 4·41 5·92 8·16 4·02 6·26	30·52 29·54 29·96 30·47 29·48 30·00	57·0 35·4 43·7 49·0 34·2 44·2	29 1 9 11 0 7	— — — — — —								

doubtless due in great part to changes occurring whilst the observations were in progress.

Possible Influence of Pattern of Instrument.

§ 10. The conclusions in the previous paragraph refer as yet only to the portable electrometer. They can be extended to the electrograph records only if we are able to show that a fairly uniform ratio exists between the potential obtained with the water-dropper at a fixed station and that obtained with the portable electrometer at one or other of the stations A to E.

The position of the water-dropper was maintained undisturbed, barring accidents, throughout the observations. It thus suffices to compare the curve readings with the corresponding ones with the portable electrometer at station A. The curves were accordingly measured at the mean times of each set of observations. The ratios of the individual readings to those at station A were calculated, and results obtained analogous to those in Table II. It will suffice for our present object to consider the results analogous to those in Table III.

Table VII.

Percentage Deviations from the Means (Electrograph/Portable).

	Series of observations.			
	I.	II.	III.	IV.
Percentage deviations	28	30	35	28

The spot where the jet breaks up resembles B more closely than any other station, and shares its low potential. Further, the electrograph curves are read to the nearest 5 volts only, so that uncertainties in the reading are even more important than with the portable, read to the nearest 1 or 2 volts, at station B. Thus, the results in Table VII are, at least, not conspicuously worse than those in Table III. As a matter of fact, the results in Table VII were, I believe, somewhat prejudiced by a variation in the water jet throughout the day (see § 11). Supposing this defect removed, the evidence points to the conclusion that the diurnal, and possibly the annual, variations got out with the water-dropper situated in the Observatory, and the portable electrometer at station A, may be expected to be in good accord, assuming the conditions under which each instrument works to be maintained uniform.

Attention was also directed to the possibility of the two different patterns of instrument being differently affected by the same climatic conditions. Each series of observations—the forenoon and afternoon observations of series II and III being treated separately—was

arranged in descending order of some one meteorological element. Suppose there to be $2n$ or $2n+1$ observations in the series (or half series); the mean values of the ratios borne by the electrograph readings to the corresponding ones with the portable electrometer at station A were calculated for the first n and the last n instances separately. Supposing r_1 , r_2 , and r to denote the mean ratios for the first n , the last n , and the whole $2n$ (or $2n+1$) observations, then

$$\frac{1}{2}\{(r_1-r_2)/r\} \times 100$$

may be regarded as the average percentage deviation of the two groups from the mean. Table VIII gives the value of this quantity in the case of the three meteorological elements from which a differential effect was most feared.

Table VIII.

Value of $\frac{1}{2}\{(r_1-r_2)/r\} \times 100$.

Series of observations.	Times of observations.	Meteorological element considered.		
		Vapour density.	Sunshine.	Wind velocity.
I	Day	-15	..	- 8
II	Forenoon	-12	+ 8	+ 8
	Afternoon	+ 1	+ 4	+12
III	Forenoon	-20	+14	+22
	Afternoon	-15	+15	-16
IV	Day	-15	..	< 0½

A plus sign denotes that when the meteorological element in question was above its mean the water-dropper was more than usually effective, relative to the portable electrometer; a minus sign implies the contrary.

The evidence in the case of wind velocity is so contradictory that we can safely assume that no uniform differential action exists.

In the case of the two other elements the evidence is more consistent, and it is possible that a small differential action may exist. It looks as if much moisture, when not counterbalanced by a contrary action of sunshine, tends slightly to pull down the reading of the water-dropper relatively to that of the portable electrometer. The phenomenon, supposing it to exist, might be ascribed to a loss of efficiency in a water jet when the vapour in the air increases, and a similar loss in a flame collector during bright sunshine. But an influence at least as likely is that of moisture, during damp weather, on the insulation of the electrograph.

Defects in Water-dropper and Portable Electrometer.

§ 11. Both instruments aim at communicating the potential at a fixed point in the air to an insulated conductor by detaching from a mass in electrical connexion with the conductor a continuous succession of small elements. It is at least doubtful whether either instrument can ever fully accomplish its object. If the object were so far accomplished that a constant fraction of the true potential were recorded, the deficiency of this fraction from unity would hardly be of primary importance in dealing with diurnal or annual variation; but if the fraction has itself a diurnal or secular variation it is a very different matter.

In the water-dropper a uniform state of insulation of the water-can, electrometer needle, and connecting wire is not easy. Absolute insulation, when a voltage runs up to hundreds, is a somewhat ideal state of perfection. When the insulation is indifferent, the recorded may fall far below the true potential. The water jet, so to speak, is running up the potential, leakage from the can, wire, &c., running it down. The resultant effect depends on a variety of things, *e.g.*, the rate at which the air potential is changing and the supply of water particles. Unless the potential is unusually steady, and the insulation exceptionally good, one may expect higher potential records with a copious jet than with a restricted one.

In the portable electrometer there is similarly some ground for expecting the potential recorded to be influenced by the rate of combustion of the fuse.

The uniformity of the disintegrating mass may also be of importance. With a water-dropper there ought not to be much uncertainty on this ground, but as electrometer fuses are articles of commerce uniformity in their material and condition is less easily ensured.

There is a final source of uncertainty common to the two instruments as commonly used. With the water-dropper the spot where the jet breaks up is apt to be slightly influenced by variations in the water pressure. When the issuing jet makes as usual an angle with the wall of a building, the consequences, as appears from Table II, are likely to be appreciable. It was a recognition of this fact that led to the taking of the observations with the portable electrometer at two nearly fixed hours, the afternoon one when the can was nearly full, the forenoon one when it was about half empty.

The corresponding defect with the portable electrometer is the burning down of the fuse. When the fuse is used in a vertical position, the height of the spot whose potential is being measured diminishes as the fuse burns, and with the height the potential falls off.

No direct comparison of the readings of the two instruments at one

and the same spot was attempted during the observations, as it seemed undesirable to interrupt the continuity of the electrograph records. All that § 10 shows is that during any one series of observations the fractions of the true potential picked up by the two instruments stood to one another in a fairly constant ratio. The presumption, certainly, is that neither fraction altered much throughout the few weeks covered by any one of the four series of observations.

It is, however, I regret to say, perfectly certain, from the data on which § 10 is based, that one at least of the two instruments varied very considerably in the course of a year and possessed an appreciable diurnal variation.

§ 12. On the discovery of these defects it became not only justifiable but necessary to subject the water-dropper itself to direct experiments. These have led to my proposing certain alterations which are now in process of execution. They aim at bringing the water-can and electrometer close together, and at maintaining a more uniform water pressure than heretofore.

It appears also desirable to check the working of the apparatus in some way involving the arrival at exact numerical results. The following operations A, B, C will, it is hoped, prove sufficient. The operation C need not be performed so frequently as A or B.

A. Charge the quadrant electrometer needle to a high potential, and observe the rate of leakage over a fixed range by timing the motion of the spot of light across a scale with—

- (1) the wire connexion to the water-can complete, but the jet not flowing ;
- (2) the wire connexion broken at the can ;
- (3) the wire connexion broken at the electrometer.

B. As a substitute, or as subsidiary to A. Connect a portable electrometer to the water-can, and, with the jet flowing, observe the potential recorded by the portable, when—

- (1) the can is connected as usual to the quadrant electrometer ;
- (2) the connexion is broken at the quadrant electrometer ;
- (3) the connexion is broken at the can.

C. Take a sufficient number of observations at a fixed station outside with a portable electrometer, at or near two fixed hours a day, so chosen that at one hour the can is almost full, whilst at the other it is at least half empty.

The use to be made of the results is obvious.

I should also recommend any one using a portable electrometer to test its scale value from time to time by comparison with an absolute electrometer or a large battery of constant cells. It is well to lay in a new stock of fuses before exhausting one's supply, and to compare the old and new fuses by taking observations in rapid succession with samples of the two at a fixed station.

PART II.

Application of Results to Theories of Atmospheric Electricity.

§ 13. It seemed desirable to consider what bearing the special experiments might have on the general facts and theories of atmospheric electricity. In this investigation special attention has been given to the possible influence of aqueous vapour on electrical potential, on account of the important researches of Exner, and of Elster and Geitel.

Theories of Exner and of Elster and Geitel.

§ 14. Exner has advanced the view that the potential gradient in the open, dV/dn in his notation, and the density q_0 of aqueous vapour simultaneously present in the atmosphere, are connected by a formula

$$dV/dn = \text{constant} \div (1 + kq_0),$$

where k is apparently a constant, the same at all places and at all seasons of the year. Exner, I believe, limited his observations, and presumably the application of the formula, to days comparatively quiet and free from clouds. To test the formula he arranged his observations in groups, according to the amount of vapour present, and compared the mean vapour density—measured in grams per cubic metre—with the mean potential gradient, measured in volts per metre of height above the ground. In the ‘Wien. Sitz.,’ Bd. 99, p. 618, he gives a table including results from Vienna, Wolfenbüttel, St. Gilgen, and India, in which the vapour densities vary from 1.7 to 23.5. The table unquestionably shows a diminishing mean potential gradient accompanying an increasing mean vapour density. For values of q_0 from 12.4 and upwards, however,—including all the Indian and most of the St. Gilgen observations—the change in dV/dn is somewhat small and irregular. An earlier, and somewhat similar, but less extensive table by Exner will be found on p. 434 of ‘Wien. Sitz.,’ Bd. 96.

For information as to Elster and Geitel’s work I am mainly indebted to a long paper by them in the ‘Wien. Sitz.,’ Bd. 101, p. 703, 1892. During 1888–91 they took an extensive series of observations on quiet days at Wolfenbüttel. If I follow their explanations, they took eye observations some ten times a day with an electrometer, in which flame from a lamp acts as collector, and deduced the mean value of the potential gradient dV/dn for the day. They compare these potential gradients grouped according to the value of the vapour density with Exner’s formula, taken to be

$$(dV/dn) \text{ (in volts per metre) } = 1410/(1 + 1.15q_0),$$

where q_0 is measured as above in grams per metre. They give an abstract of the results on their p. 742, in the shape of a table which I reproduce.

Elster and Geitel's Table III (*loc. cit.*, p. 742).

$q_0 =$	1·6	1·9	2·5	3·7	4·6	5·6	6·5	7·6	8·4	9·4	10·6	13·5
dV/dn (observed) =	502	430	400	318	252	137	184	148	112	115	118	121
„ (calculated) =	496	442	364	268	224	189	166	145	133	119	107	85

It would appear that Elster and Geitel, like Exner, found large departures from the mean dV/dn of a group amongst its individual members.

§ 15. Elster and Geitel next proceed to investigate a possible connexion between the potential gradient and the intensity of that species of solar radiation which dissipates a negative charge on an insulated sphere of polished zinc. If I understand them rightly, they measured the mid-day intensity J of this radiation, and compared the potential gradient with several formulæ in which the variable was either J or Jf , where f is a "Beleuchtungsfactor," equal apparently to (possible hours of sunshine)/12. Taking a formula $dV/dn = 110 + 360a^{-J}$, where $\log a = 0\cdot0100$, they give the following comparison of the results of observation and theory:—

$J =$	2·9	5·8	9·1	21·4	58·8	77·1	113·7	121·9	181·3	194·5	268·4
dV/dn (observed) =	447	430	368	325	198	181	138	126	120	106	102
„ (calculated) =	447	425	402	330	203	171	136	132	116	114	111

The agreement seems better than in the case of Exner's formula, and Elster and Geitel seem strongly inclined to regard ultra-violet radiation as the direct cause of variations of potential on normal quiet clear days. They consider apparently that there are only two defective links in the chain of evidence, viz.:—

- (1) absolute proof that the earth is electrified negatively;
- (2) proof that there is a sufficient supply at the earth's surface of materials susceptible to the influence of ultra-violet light.

There are, of course, numerous other theories of atmospheric electricity, but none, so far as I know, admits of numerical comparison with observation.

Method of Treating Kew Observations.

§ 16. In discussing the Kew observations I have in general employed a method differing from the grouping system of Exner and Elster and Geitel, and have also treated the several series separately. It is clear from data mentioned by Exner that the potential gradients for individual members of his groups varied in some instances largely from the mean; and it was soon obvious that the same phenomenon would present itself if any similar treatment were applied to the Kew results. This is undesirable, because by varying the limits of the groups the accordance of the results with a particular formula may be much improved, or the reverse. However impartially, so to speak, the lines may be drawn, there is undeniably a risk of introducing some fictitious result; and no critic can feel that he is in a position to judge of the results until he has examined for himself the circumstances of the grouping, a labour he naturally shrinks from. Again a wide range of such an element as vapour density can be obtained at a particular place only by combining results from all seasons of the year. This brings us to a second question. Electrical potential gradient has like vapour density, sunshine, and temperature, a large annual variation, only, unlike these elements, it is highest in winter. It is thus obvious that when observations from all seasons of the year are treated promiscuously, there is almost sure to be a marked association of high potential with low vapour density, little sunshine, and low temperature; and a judiciously selected formula which makes potential diminish as any one of these elements increases is certain to show some approach to agreement with observation. It is thus desirable to compare together observations from a limited portion of a year, or, even better, from the same season of a series of years. Similar considerations show an advantage in treating separately results from different hours of the day. The isolation of particular seasons and hours has the disadvantage of reducing the number of observations compared together. This is, however, partly compensated for by the greater homogeneity of the material. It also enables one in some cases to compare readily the mean potential gradients which answer at different seasons or hours to like values of some one meteorological element (see § 23).

Anticipation of some Criticisms.

§ 17. The Kew observations were not limited to quiet, comparatively cloudless days, in the same way as the observations of Exner and Elster and Geitel seem to have been. It may thus not unlikely be supposed that the Kew results are affected by a variety of disturbing causes, which diminish their intrinsic value and their suit-

ability for comparison with other results free from these extraneous effects.

As to intrinsic value, there are, at least in England, seasons of the year when a nearly cloudless day is exceptional. For instance, during November and December, 1894, in ten days out of eighteen, on which observations were taken a little before noon, no bright sunshine was recorded up to the hour of observation. At such a season, if one confined one's attention to nearly cloudless days, hardly any data would be obtained, and they might not unreasonably be regarded as abnormal.

As to the disturbing action of clouds, this is no doubt in some cases very large; but with clouds of this character the influence may be considerable when they cover only a small fraction of the sky, and probably, in some cases, even when they are below the horizon. Thus on one occasion at Kew, when part of the sky was covered by a thundercloud—so distant that only one or two faint lightning flashes were detected—sudden changes of potential of thousands of volts from negative to positive and back again were observed on the roof, whilst the sun shone at intervals. The sudden alternations of potential doubtless accompanied flashes of lightning, but no rain was falling anywhere near, and possibly an observer a few miles away might have regarded the day as an ideal quiet one. Again, there are other forms of clouds whose influence seems not unlikely to be much less than that of invisible vapour in motion nearer the ground. The mere interception of sunlight by cirrus clouds or detached masses of cumulus, if we may judge from some few experiments at Kew, has little if any effect.

It should also be borne in mind that wind velocity and amount of cloud must both have varied appreciably from day to day, and even throughout the individual days of Exner's experiments. Some one—I forget who—defined a "quiet" day as one in which the flame of Exner's electrometer was not blown out. All the days of the Kew observations satisfied, of course, this definition, if one is allowed to substitute the portable electrometer for Exner's, yet on one occasion the anemometer was recording a mean velocity of forty miles an hour.

If aqueous vapour, as Exner supposes, is the sole, or even the dominant, agent in producing changes in potential, its activity can hardly be confined to days when there is little cloud, and the wind is low.

§ 18. As regards Elster and Geitel's theory, the data available for criticism are, I admit, defective, inasmuch as no measurements are taken at Kew of the dissipative effect of sunlight on negative electricity. I presume, however, that bright sunshine—such as the Campbell-Stokes instrument records—always possesses this power,

though doubtless in very variable degrees at different seasons. Solar radiation occurring after an observation is taken, clearly cannot affect it. Thus the data got out as to the amount of bright sunshine recorded prior to the observations must, I think, bear fairly directly on Elster and Geitel's theory. If it be true, the potential gradient must, I think, fall conspicuously as the number of hours of previous sunshine increases.

§ 19. An objection of a different kind is the proximity of the Kew Observatory to London. This objection has already been urged against Greenwich by investigators* whose theories do not harmonise with the results obtained there. A weekly period exists, they say, in the Greenwich electrograph curves, and this, they assume, can arise only from a weekly fluctuation in the amount of smoke, due to our insular habits of keeping Sunday. If, for a moment, we suppose the phenomenon and explanation both true—a pretty large assumption—there seems a wide step to the conclusion that results so affected are useless. I do not myself see that they need lead to erroneous conclusions, unless one is dealing with a cycle whose period is seven days, or a multiple thereof, which a lunation, for instance, is not.

In the present instance I would point out that the prevailing winds during each one of the series of observations were from directions included between N.N.W. and S., and that as Kew Observatory is some miles to the *west* of London, while the manufacturing districts are mainly in the east, it is difficult to see how London smoke could affect the results. The Observatory, I should add, is situated in a large open park to the immediate west of the extensive Kew Gardens.

Even if the prevailing winds had been easterly, I question whether smoke would have exerted an appreciable influence. The analysis above mentioned of the electrograph results for 1880, by the late Mr. Whipple, seems to show that if any relation existed then between electric potential and wind direction, it varied with the season of the year; this would hardly have occurred if smoke present in east winds had an appreciable effect.

Tables of Results.

§ 20. In discussing the observations, I have decided to commence by incorporating the actual details in a series of tables. This will enable any one to judge for himself whether the conclusions finally arrived at are in accordance with the facts. The first eight tables give full particulars of the results. The arrangement is not

* See pp. 42—43 of offprint of paper by Ekholm and Arrhenius in 'Bihang till k. Svenska Vet.-Akad. Handlingar,' Band 19, Afd. 1, No. 8, Stockholm, 1894.

Table IX.

Results for November—December, 1894, forenoon, about 11.50 A.M.

Volts observed at stations						Vapour, grams per cubic metre.	Relative humidity.	Hours of sunshine.	Temperature, Fahrenheit.	Barometer, 29 inches +	Wind velocity, miles per hour.
A	B	C	D	E							
264	66	464	640	—		7.33	85	0	48.0	1.11	10
232	66	552	643	776		7.69	96	0	45.5	1.04	10
208	36	496	612	670		5.66	92	0.2	38.6	1.38	13
198	48	408	494	620		5.52	79	2.8	42.0	1.60	11
196	38	508	516	728		9.30	99	0	50.2	1.09	1
192	46	400	452	568		6.12	92	0	40.5	1.06	10
192	40	408	416	516		3.97	76	0.3	34.0	0.65	15
188	44	464	540	—		8.23	88	0	50.0	1.09	10
168	46	382	362	—		5.29	92	0	36.5	1.00	4
160	42	322	376	476		4.96	67	2.8	43.1	0.98	19
152	58	362	280	—		5.76	74	2.8	44.5	1.46	14
148	32	452	428	—		7.69	96	0	45.5	0.81	3
124	32	264	292	368		5.45	67	2.5	45.9	1.02	29
124	26	290	250	322		4.52	66	2.3	41.5	0.34	33
116	30	320	340	574		7.62	82	0	50.0	1.15	22
106	32	198	240	264		5.63	72	1.8	44.8	0.49	20
104	35	180	152	—		5.26	75	0	42.0	1.35	7
84	22	230	280	—		5.95	88	0	41.0	0.85	6
Means..	164	41	372	406	535	6.22	83	0.86	43.5	30.03	13.2

Table X.
Results for November—December, 1894, afternoon (times various).

Volts observed at stations						Vapour, grams per cubic metre.	Relative humidity.	Hours of sunshine.	Temperature, Fahrenheit.	Barometer, 29 inches +	Wind velocity, miles per hour.
A	B	C	D	E							
192	42	424	452	—		6.60	84	1.2	45.0	0.91	8
164	44	416	364	404		5.47	67	2.2	46.2	0.53	18
160	58	404	530	—		4.88	94	0	33.5	0.70	2
148	40	276	308	—		7.14	87	0.2	46.6	1.09	12
114	28	270	212	—		6.22	79	0.5	45.0	1.01	20
108	28	120	254	—		6.47	92	0	42.0	1.26	0
104	32	182	195	—		5.03	89	0	35.9	1.48	3
Means..	141	39	299	331		5.97	85	0.59	42.0	30.00	9

Table XI.

Results for March—April, 1895, forenoon, about 10.30 A.M.

Volts observed at stations						Vapour, grams per cubic metre.	Relative humidity.	Hours of sunshine.	Temperature, Fahrenheit.	Barometer, 29 inches +	Wind velocity, miles per hour.
A	B	C	D	E							
708	183	1320	1464	1688		5.50	87	1.0	39.1	1.30	3
580	215	812	847	1073		5.55	87	1.0	39.6	0.66	1
415	75	696	702	806		5.25	67	0	44.9	0.64	10
406	96	687	818	908		6.73	78	0.4	47.9	1.34	0
400	101	690	847	986		5.84	67	2.3	48.0	1.19	4
304	67	560	600	664		7.89	82	1.3	50.8	0.92	11
275	73	499	537	589		6.38	70	0.5	49.5	1.37	8
260	67	502	542	580		8.54	73	0.3	56.4	1.00	5
203	58	412	455	563		4.67	71	1.3	40.1	0.97	22
186	52	302	377	441		6.49	84	0	44.9	1.02	9
186	58	348	314	345		5.73	88	0	40.2	0.93	6
180	35	412	604	687		7.66	96	0	45.6	0.10	4
168	55	248	522	598		7.27	81	0.2	48.8	0.80	6
166	29	288	296	319		6.12	68	3.3	48.8	0.27	21
165	46	270	472	551		7.85	76	3.4	53.1	0.94	6
160	38	258	293	348		8.36	74	0.8	55.3	0.91	13
133	35	232	336	413		9.03	94	0	50.9	0.93	9
122	38	238	280	293		4.84	72	0.9	41.6	1.10	10
108	49	226	242	299		8.39	83	0	52.2	0.82	14
104	26	174	214	260		6.48	78	2.0	47.0	1.86	40
104	32	174	215	226		6.35	60	4.2	53.7	1.28	5
67	29	131	160	182		6.06	73	0	47.0	0.56	24
52	15	81	122	191		5.82	75	1.3	44.3	1.20	10
50	27	116	139	197		6.54	72	0.3	49.0	0.21	20
Means..	229	62½	403	475	550	6.64	77	1.02	47.4	29.85	10.9

Table XII.

Results for March—April, 1895, afternoon, about 4.30 P.M.

Volts observed at stations						Vapour, grams per cubic metre.	Relative humidity.	Hours of sunshine.	Temperature, Fahrenheit.	Barometer, 29 inches +	Wind velocity, miles per hour.
A	B	C	D	E							
395	145	630	1307	1530		5.61	70	2.0	46.0	0.72	18
377	90	568	1125	1351		5.93	82	1.6	43.0	0.17	13
290	70	493	595	696		6.42	69	0.7	50.0	1.35	4
264	70	499	512	621		5.63	62	2.0	49.3	0.64	13
209	64	307	368	452		6.88	59	8.3	56.5	1.08	9
177	47	394	435	485		4.69	64	1.2	43.2	1.17	6
168	64	302	348	418		6.74	65	6.8	53.1	1.27	7
168	55	328	531	580		7.70	56	9.4	61.6	0.91	6
168	41	270	290	345		5.81	63	3.7	49.4	0.43	20
151	41	313	365	397		7.67	70	2.5	54.8	0.80	23
151	44	281	293	338		8.95	66	6.0	61.0	0.92	19
149	52	261	247	339		5.22	71	4.8	43.2	1.06	11
145	52	258	383	420		7.85	76	0.3	52.9	0.89	8
130	32	240	299	371		9.19	69	1.0	60.4	0.86	11
122	27	218	212	218		6.11	65	6.8	50.3	0.08	28
122	32	261	296	348		6.32	78	1.1	46.2	0.89	1
116	35	261	313	345		5.17	60	2.4	48.0	1.92	27
110	32	168	200	217		5.61	47	10.2	57.9	1.26	14
99	41	174	447	624		6.21	79	0	45.4	0.86	14
96	15	197	212	255		8.72	86	0	52.2	0.04	21
Means..	180	52	321	439	517½	6.62	68	3.54	51.2	29.77	13.6

Table XIII.

Results for June—July, 1895, forenoon, about 10.30 A.M.

Volts observed at stations						Vapour, grams per cubic metre.	Relative humidity.	Hours of sunshine.	Temperature, Fahrenheit.	Barometer, 29 inches +	Wind velocity, miles per hour.
A	B	C	D	E							
174	39	306	354	405		9.11	57	0.5	65.7	0.87	3
141	36	204	279	357		6.33	38	5.1	66.8	1.38	5
117	27	180	186	213		12.44	78	0	65.7	1.33	8
114	30	207	255	300		9.52	50	4.3	70.6	1.02	13
105	21	168	204	246		12.62	81	0	65.2	0.68	7
105	18	204	234	276		8.58	45	5.5	71.0	1.11	7
102	30	189	225	279		12.01	66	0.3	69.5	1.41	6
102	36	234	291	351		8.58	60	1.5	62.8	1.18	5
99	27	153	177	186		8.50	59	1.2	63.1	0.56	24
99	27	180	264	291		9.54	78	0	57.5	0.68	7
90	24	180	180	216		7.88	48	5.7	66.0	1.25	11
90	15	183	207	264		12.13	60	3.9	72.9	1.14	6
87	18	150	225	270		8.76	47	2.1	66.6	0.93	13
84	24	147	150	168		7.96	55	4.0	63.0	0.95	9
81	15	129	153	201		9.38	53	4.5	68.8	0.91	16
36	9	69	132	156		8.36	44	4.0	71.0	0.91	13
Means..	102	25	180	220	261	9.48	57	2.66	66.6	30.02	9.6

Table XIV.

Results for June—July, 1896, afternoon, about 4.30 p.m.

Volts observed at stations						Vapour, grams per cubic metre.	Relative humidity.	Hours of sunshine.	Temperature, Fahrenheit.	Barometer, 29 inches +	Wind velocity, miles per hour.
A	B	C	D	E							
162	45	270	300	345		9.74	53	11.6	70.0	1.27	10
147	39	294	462	498		10.81	70	2.8	65.1	0.62	22
138	33	237	258	279		10.19	61	5.1	67.2	0.58	18
120	28	204	296	357		9.78	60	1.2	66.5	0.69	4
117	33	183	222	246		9.87	54	5.0	69.2	0.69	27
114	36	159	183	225		9.61	51	9.8	70.7	1.26	15
111	28	180	186	222		13.26	68	1.3	72.0	1.40	4
105	33	120	174	174		6.90	32	11.1	76.2	1.28	10
102	36	162	180	216		8.17	40	8.0	73.4	0.91	8
96	30	150	168	207		9.30	38	11.5	79.1	1.04	11
81	30	147	153	204		8.68	41	6.4	74.4	0.74	15
78	15	150	159	174		10.45	62	7.0	67.3	0.85	16
48	24	75	120	138		10.31	50	7.5	74.2	0.94	18
27	12	27	30	30		11.62	58	3.5	73.0	1.36	10
Means ..	103	30	168	206	237	9.91	53	6.56	71.3	29.97	13.4

Table XV.

Results for October—November, 1895, forenoon, about 10.30 A.M.

Volts observed at stations						Vapour, grams per cubic metre.	Relative humidity.	Hours of sunshine.	Temperature, Fahrenheit.	Barometer, 29 inches +	Wind velocity, miles per hour.
A	B	C	E	F							
830	115	1452	1785	2362		7.08	97	0	42.8	1.42	5
603	110	1160	1323	2000		4.02	77	0.5	34.2	0.75	5
341	75	588	951	1540		4.75	83	0	36.4	0.55	3
296	42	653	893	1420		4.41	75	1.7	37.0	1.13	6
290	72	652	879	1456		5.85	93	0	39.0	1.52	0
278	49	493	725	1058		6.28	75	2.2	47.2	1.47	11
183	30	247	391	638		7.81	87	0.1	49.0	0.88	10
174	52	296	449	783		5.35	79	0	41.0	0.57	13
168	26	278	391	638		8.16	93	0	48.1	1.38	0
162	44	464	580	615		4.65	75	1.3	38.8	0.51	8
110	32	226	302	806		9.07	76	1.2	57.0	0.73	29
90	12	200	426	751		4.81	87	0	35.4	0.81	8
30	12	53	110	133		7.11	77	0.4	49.3	1.16	18
Means..	273	52	520	708	1092	6.10	83	0.57	42.7	29.99	8.9

Table XVI.

Results for October—November, 1895, afternoon, about 4.30 P.M.

Volts observed at stations						Vapour, grams per cubic metre.	Relative humidity.	Hours of sunshine.	Temperature, Fahrenheit.	Barometer, 29 inches +	Wind velocity, miles per hour.
A	B	C	E	F							
458	104	1027	1227	2053		7 00	72	1 6	51 0	1 46	7
319	64	580	870	1433		7 90	89	0 1	48 5	0 96	5
247	50	328	516	826		5 40	80	5 2	41 0	0 71	6
215	55	435	595	879		5 26	71	7 4	43 7	0 50	4
197	52	377	638	971		5 38	74	0	42 7	0 54	14
194	35	368	458	719		6 85	75	7 6	49 0	1 47	8
180	39	397	568	957		5 72	62	3 1	49 7	1 25	9
174	15	290	516	597		7 76	86	3 0	49 0	1 42	7
157	46	322	537	986		4 90	69	3 4	42 7	1 05	7
29	12	52	93	122		5 58	75	4 4	43 5	0 48	8
Means..	217	47	418	602	954	6 17	75	3 58	46 1	29 98	7 5

chronological, but in descending order of the voltages observed at station A. In all the tables the humidity of saturation is taken as 100.

§ 21. I have divided the results in each of the previous eight tables into two groups, according to the order of voltages at station A. The two groups are equal in number of constituents, except as regards Table XV, where, following a marked line separating the voltages, I have included six in the first group, and seven in the second; and Table X, where I have included four in the first group, and three in the second. In the last-mentioned case, the best line of demarcation is doubtful, and, on account of this, and the small number of constituents, little weight can be attached to the results.

It may seem arbitrary to determine the groups by reference to station A exclusively. It is, however, the station least influenced by buildings, and best fitted for accurate readings, while B is the worst. Also it will be seen that if one had adopted either C, D, or E as the standard station, or had taken a mean from all the stations, whilst the order of the constituents would in some tables have been considerably affected, the groups themselves would have suffered little or no change.

Table XVII gives the mean potentials at station A for each group in the several series of observations, with the corresponding mean values of the meteorological elements.

Table XVII.

Series of observations.	Time.	Potential at Station A.		Vapour, grams per metre.		Relative humidity.		Hours of sunshine.		Temperature, Fahrenheit.		Barometer, 29 inches +		Wind velocity, miles per hour.	
		1st n.	2nd n.	1st n.	2nd n.	1st n.	2nd n.	1st n.	2nd n.	1st n.	2nd n.	1st n.	2nd n.	1st n.	2nd n.
I {	Forenoon...	204	124	6.57	5.87	89	76	0.37	1.36	42.8	44.3	1.11	0.94	9.3	17.0
	Afternoon..	166	109	6.02	5.91	83	87	0.90	0.17	42.8	41.0	0.81	1.25	10.0	7.7
II {	Forenoon...	342	117	6.35	6.93	79	76	0.68	1.37	45.6	49.3	0.95	0.74	6.9	14.8
	Afternoon..	237	124	6.31	6.93	66	70	3.82	3.26	50.7	51.7	0.85	0.68	11.9	15.4
III {	Forenoon...	120	83	9.90	9.06	59	56	2.15	3.17	67.2	66.1	1.12	0.92	6.8	12.4
	Afternoon..	130	77	10.47	9.35	60	46	5.26	7.86	68.7	73.9	0.93	1.02	14.3	12.6
IV {	Forenoon...	440	131	5.40	6.71	83	82	0.73	0.43	39.6	45.5	1.14	0.86	5.0	12.3
	Afternoon..	287	147	6.19	6.16	77	73	2.86	4.30	45.4	46.8	0.83	1.13	7.2	7.8

§ 22. A discussion might be based on the previous nine tables alone. Partly, however, to satisfy those who prefer a grouping system like that of Exner, I add further tables, in which the observations are arranged in groups, according to the magnitude of some one meteorological element. In dealing with vapour density, barometric pressure and wind velocity, separating lines have been drawn at fixed values of the element considered. In the case of vapour density the limits required to be altered with the season. In the case of barometric pressure and wind velocity it was deemed sufficient to draw only one separating line, which answered, it will be seen, very nearly to the mean value. In dealing with sunshine and temperature,* the division has been into equal, or as nearly equal, groups as possible. After the tables follows a discussion, which embraces Tables IX to XVII as well.

Table XVIII.

Arrangement according to Vapour Density.

Series of observations.	Vapour grams per metre.	Forenoon.			Afternoon.		
		Number of observations.	Mean vapour density.	Mean potential at A.	Number of observations.	Mean vapour density.	Mean potential at A.
I {	>6	7	7·71	191	4	6·61	141
	<6	11	5·27	147	3	5·13	143
II {	>7	8	8·12	185	6	8·35	140
	7 to 6	8	6·39	170	6	6·45	168
	<6	8	5·40	333	8	5·46	219
III {	>10	4	12·30	104	6	11·11	92
	10 to 9	4	9·39	117	5	9·66	122
	<9	8	8·12	93	3	7·92	96
IV {	>6	6	7·58	266	4	7·38	286
	<6	7	4·83	279	6	5·37	171

* The $n + 1$ th constituent in the forenoon observation of Series IV is omitted, as it was doubtful whether to group it with the first n or last n .

Table XIX.

Arrangement according to Hours of Sunshine.

Series of obser- vations.	Sunshine.	Forenoon.			Afternoon.		
		Num ber of obser- vations.	Mean sunshine hours.	Mean potential at A.	Number of obser- vations.	Mean sunshine hours.	Mean potential at A.
I {	Most	8	1·94	158	4	1·03	154
	Least	10	0	169	3	0	124
II {	Most	12	1·90	256	10	6·09	151
	Least	12	0·14	203	10	0·99	209
III {	Most	8	4·62	93	7	9·50	101
	Least	8	0·70	111	7	3·61	106
IV {	Most	7	1·06	237	5	5·60	168
	Least	6	0	315	5	1·56	266

Table XX.

Arrangement according to Temperature.

Series of obser- vations.	Tempera- ture.	Forenoon.			Afternoon.		
		Number of obser- vations.	Mean tempera- ture.	Mean potential at A.	Number of obser- vations.	Mean tempera- ture.	Mean potential at A.
I {	Highest	9	47·2	170	4	45·7	154
	Lowest	9	39·9	159	3	37·1	124
II {	Highest	12	51·4	191	10	56·1	145
	Lowest	12	43·5	267	10	46·4	216
III {	Highest	8	69·6	95	7	74·6	81
	Lowest	8	63·6	109	7	68·0	125
IV {	Highest	6	48·9	266	5	49·4	265
	Lowest	6	36·8	297	5	42·7	169

Table XXI.

Arrangement according to Barometric Pressure.

Series of observation.	Barometric pressure, in inches.	Forenoon.			Afternoon.		
		Number of observations.	Mean pressure.	Mean potential at A.	Number of observations.	Mean pressure.	Mean potential at A.
I {	> 30	12	30·20	178	4	30·21	119
	< 30	6	29·69	136	3	29·71	172
II {	> 30	9	30·20	279	6	30·20	184
	< 30	15	29·64	199	14	29·58	179
III {	> 30	8	30·23	108	6	30·27	103
	< 30	8	29·81	96	8	29·75	104
IV {	> 30	6	30·35	315	5	30·33	233
	< 30	7	29·69	238	5	29·64	201
All combined {	> 30	35	30·23	212	21	30·25	160
	< 30	36	29·69	173	30	29·65	162

Table XXII.

Arrangement according to Wind Velocity.

Series of observations.	Wind velocity, miles per hour.	Forenoon.			Afternoon.		
		Number of observations.	Mean velocity.	Mean potential.	Number of observations.	Mean velocity.	Mean potential.
I {	10 or > 10	13	16·6	174	3	16·7	142
	< 10	5	4·2	140	4	3·2	141
II {	10 or > 10	11	17·7	159	13	17·8	179
	< 10	13	5·1	289	7	5·9	183
III {	10 or > 10	6	15·0	85	11	15·6	101
	< 10	10	6·3	112	3	5·3	111
IV {	10 or > 10	5	16·2	155	1	14·0	197
	< 10	8	4·4	347	9	6·8	219
All combined {	10 or > 10	35	16·6	151	28	16·7	145
	< 10	36	5·1	232	23	5·7	180

Vapour Density.

§ 23. In Tables XVII and XVIII the forenoon observations of series IV, and both forenoon and afternoon observations of series II, support Exner's theory to a certain extent, inasmuch as they decidedly, on the whole, associate higher potential with lower vapour density. The forenoon observations, however, of series I lead in both tables to exactly the opposite result. Also in Table XVII, in five cases out of eight, the higher potential is associated with the higher vapour density. In some instances, *e.g.*, the afternoon observations of series III and IV, Tables XVII and XVIII lead to diametrically opposite conclusions. The following are instances of corresponding means of vapour density and potential, culled from the several tables. In Table XVIII, 8.12 occurs with both 185 and 93; in Table XI, 6.64 with 229; in Table XII, 6.62 with 180; in Table XVII, 6.57 with 204, 6.35 with 342, and 6.31 with 237; in Table XVIII, 6.61 with 141, 6.45 with 168, and 6.39 with 170. Again, in Tables XV and XVI we find 6.10 associated with 273, and 6.17 with 217. Lastly, in Table XVIII we have the following combinations, 5.46 with 219, 5.40 with 333, 5.37 with 171, and 5.27 with 147.

In the face of such results, it seems difficult to believe in any intimate and uniform connexion whatsoever between potential gradient and vapour density.

Relative Humidity.

§ 24. No special table is devoted to this. In Table XVII no less than six sub-cases out of eight associate higher relative humidity with higher potential. It will be noticed, however, that in three out of the six sub-cases referred to the differences between the mean humidities answering to the two groups are smaller than in either of the two sub-cases which associate higher relative humidity with lower potential. With the exception of the forenoon observations of series I, and the afternoon observations of series III, the differences between the mean relative humidities in the two groups are very small. Thus, on the whole, the evidence in favour of any distinct association of higher relative humidity and higher potential is insufficient.

Sunshine.

§ 25. There is in both the Tables XVII and XIX a balance of evidence in favour of a connexion of low potential with long previous sunshine. Out of eight sub-cases in each table, five favour this connexion in Table XVII, and six in Table XIX. The only sub-case in which the tables agree in associating higher potential with longer previous sunshine is the afternoon observations of series I, and, for

reasons already mentioned, this is not an important exception. There is thus a certain amount of general support to Elster and Geitel's theory. An examination, however, of numerical details does not seem favourable to any such intimate connexion between sunshine and potential, as the formula suggested by them would imply.

Taking, for instance, Table XIX, we notice in series III that, in the afternoon, a mean potential of 106, answering to a mean of 3·6 hours' sunshine, falls only to 101 when the hours of sunshine rise to 9·5. Again, in the forenoon observations of the same series, the mean hours of previous sunshine increase fully six times, while the potential falls only from 111 to 93.

The afternoon observations of series II are a striking illustration of the diverse conclusions to which the different methods adopted in Tables XVII and XIX may lead.

Temperature.

§ 26. The forenoon observations of series IV, and both forenoon and afternoon observations of series II associate high potential with low temperature in both Tables XVII and XX; and the balance of evidence is unquestionably in this direction. The only sub-case in which the two tables agree in associating higher potential with higher temperature is the afternoon observations of series I, which, as already explained, is the least important of the eight instances.

On the whole, the evidence in favour of a connexion of high potential with low temperature is just about as strong as that in favour of a connexion of high potential with little previous sunshine.

Barometric Pressure.

§ 27. Higher potential is associated with higher pressure in the forenoon observations of each of the four series both in Tables XVII and XXI. In the afternoon observations, however, the higher potential is associated with the lower pressure in three cases out of four in Table XVII, and in two cases out of four in Table XXI. The phenomenon, in short, is an apparently clear association of high potential and high barometric pressure in the forenoon, and an apparent absence of any connexion in the afternoon.

Wind Velocity.

§ 28. A somewhat striking similarity exists here to the phenomena observed in the case of barometric pressure.

In both the Tables XVII and XXII there is in the forenoon results a conspicuous association of high potential with low wind velocity. In Table XXII, it is true, series I observations form an exception.

but it is rather apparent than real. For if, instead of ten, we adopt eleven miles an hour as limiting value for the velocity, we get in that instance two equal groups with the following results:—

Group.	Mean velocity.	Mean potential.
1st	19·6	153
2nd	6·8	175

Higher potential is here associated with lower velocity, and, as the groups are equal, the result is presumably a fairer representation of the facts than that afforded by Table XXII.

Whilst the association of high potential with low wind velocity in the forenoon seems thus conspicuous, there is in the afternoon no certain evidence of any such connexion. Thus, in Table XVII, higher potential is associated as often with higher as with lower velocity; and in Table XXII, whilst higher potential is associated with lower velocity in three sub-cases out of four, the differences between the mean potentials for the first and second groups are small. In series III observations the difference is also very uncertain. If, for instance, we divide these observations into two equal groups, by taking 15 as separating value for the velocity, we obtain for each group identically the same mean voltage, 103, though the mean velocities for the two groups are respectively 18·7 and 8·1.

In Table XXII the figures obtained by combining all four series of observations, afford an excellent example of what may happen when results, from all seasons of the year, are treated promiscuously. The individual series, as we have seen, show no clear association of high potential with low velocity in the afternoon observations, but, when the four series are combined, such an association seems conspicuous. The phenomenon, in reality, is mainly due to the comparatively large number of instances in which the velocity happened to be high during the season when the potential was at its minimum.

General Summary of bearing of Results on Theory.

§ 29. A comparatively small number of observations may be sufficient to disclose defects in an existing physical theory, and yet be inadequate to warrant the promulgation of a positive opinion as to the true theory. This is the most satisfactory point of view from which to regard the facts presented here. They are, in my opinion, sufficient to show the incompleteness of any theory which assumes simultaneous values of potential and any single meteorological element to be so intimately connected that the value of the one can be deduced, as a rule, from that of the other without taking into account other important influences. On the other hand, they are not sufficiently varied to justify the conclusion that the connexions

traced in §§ 25 to 28 between low potential and long previous sunshine, high temperature, low barometric pressure, and high wind velocity constitute the normal state of matters at every station, irrespective of the hour or the season. Provisionally I should prefer to regard these associations as possibly accidental, even at Kew, but believe they indicate the lines on which more exhaustive inquiries might profitably proceed.

Another possibility indicated by these associations, viz., that the potential tends to be higher during anticyclonic than during cyclonic weather seems also worthy of attention. An attempt was indeed made in the present instance to check this conclusion directly by reference to the weather reports of the Meteorological Office. The published data relate, however, to 8 A.M. and 6 P.M.; so that, on a considerable number of occasions the nature of the isobars at the hours of the observations was uncertain. Taking the remaining instances, I calculated the mean potential for the cyclonic and anticyclonic conditions separately for each one of the four series, treating the forenoon and afternoon observations apart, except in the case of the first series. In five cases out of the seven thus presented, the mean potential for the anticyclonic group exceeded that for the cyclonic. There is thus something to be said for the hypothesis. It should be mentioned, however, that individual occurrences of high potential in cyclonic weather and of low potential in anticyclonic weather were not infrequent.

§ 30. The results of the present inquiry are, I believe, irreconcilable with Exner's theory, in so far as it connects simultaneous *individual* values of potential and vapour density. The question remains open whether the annual variations of potential and vapour density may not be related through a formula of Exner's type—

$$dV/dn = A/(1+Bq_0),$$

where A and B are constants for a given station, dV/dn and q_0 representing monthly means of potential gradient and vapour density near the ground.

Whilst the data available are far too limited for drawing a final conclusion, I think it worth while to add in Table XXIII a comparison of the results at station A—regarded as 60 inches above the ground—with those deduced from Elster and Geitel's special form of the equation

$$dV/dn = 1410/(1+1.15q_0).$$

The figures are the arithmetic means of the values for the forenoon and the afternoon hours of observation.

Table XXIII.

Series of observations.	Potential at Station A.	
	Observed.	Calculated.
I.	153	269
II.	205	249
III.	103	176
IV.	245	267

The density of aqueous vapour is a quantity having but a small diurnal variation,* and it would appear, from a table published by General Sabine† that the calculated mean potential for the day—taken as the mean of the calculated values for the 24 hours—would differ but little from that answering to only the two times, 10.30 A.M. and 4.30 P.M. Thus the calculated values in Table XXIII may be regarded as close approximations to the true calculated means for the seasons of the four observations. On the other hand, according to the table of diurnal variation of potential in the paper by Mr. Whipple already referred to, the true means obtained from observations at every hour of the day might be expected to be on an average some 10 per cent. higher than the observed values in Table XXIII. It ought, further, to be remembered that, as explained in § 4, the potential at station A must fall short of the true potential at a point 60 inches above the ground in the open, also the fraction of the existing potential picked up by the portable electrometer may be appreciably less than unity. Thus the fact that the calculated values in Table XXIII are so decidedly larger on the average than the observed is perhaps rather in favour of the formula than otherwise.

If we may judge, however, from the few data in the table, there seems some ground for the suspicion that the formula will prove to give too narrow a range.

Before concluding, I have much pleasure in acknowledging the ready and valuable help I have received from Mr. E. G. Constable, Senior Assistant at the Kew Observatory. Mr. Constable took all the electrical observations and the measurements of the meteorological curves, and gave me in addition much useful information derived from his long experience of the working of the electrograph and portable electrometer.

* A fact difficult to reconcile with the general form of Exner's theory.

† 'Roy. Soc. Proc.,' vol. 18, 1869, p. 8.

"On the unknown Lines observed in the Spectra of certain Minerals." By J. NORMAN LOCKYER, C.B., F.R.S. Received May 16,—Read June 4, 1896.

In the first note of the series "On the New Gases obtained from Uraninite," by the distillation method, I remarked* "I have already obtained evidence that the method I have indicated may ultimately provide us with other new gases, the lines of which are also associated with those of the chromosphere."

In a subsequent paper "On the Gases obtained from the Mineral Eliasite," I gave a list of several lines unknown to me, and suggested that they might indicate the existence of a new gas or gases in that mineral, and I added† "Although the evidence in favour of a new gas is already very strong, no final verdict can be given until the spectra of all the known gases, including argon, have been photographed at atmospheric pressure, and the lines tabulated. This part of the inquiry is well in hand."

The inquiry above referred to has now been completed and in the following manner:—

Photographs were taken of the spectra at atmospheric pressure of nitrogen, oxygen, chlorine, carbonic anhydride, coal gas, sulphuric anhydride, phosphoretted hydrogen, and argon, these being the gases which, from the experience thus far acquired, are likely to be associated with those given off by minerals. In addition to these, the lines of mercury, potassium, and platinum, were also photographed. The lines of platinum are always present in the spectra for the reason that the spark is passed between platinum poles, while the lines of mercury or potassium frequently appear according as the gases are collected over mercury or potash.

The spectroscope employed has a collimator and camera with object glasses of 3 in. aperture, and focal lengths of 5 ft. and 19 in. respectively. Two prisms of 60° were used, giving a length of spectrum of about 1·75 in. between K and D.

In order to facilitate the reduction of the photographs, the solar spectrum was photographed under exactly similar instrumental conditions. Micrometric measures were made of H and K, and other well-known lines throughout the spectrum, and by means of these and Rowland's wave-lengths, a curve was carefully constructed.

It may be incidentally mentioned that in the photographs of the spectra of gases at atmospheric pressure, H and K are generally present as pole lines, being probably due to an impurity of calcium in the platinum poles.

* 'Roy. Soc. Proc.,' vol. 58, p. 70.

† 'Roy. Soc. Proc.,' vol. 59, p. 3.

In each case in which K was present, the micrometer scale was set to the reading for this line, and the photograph to be reduced then adjusted until the K line was under the cross-wires of the micrometer. Each line in the spectrum was then in turn brought under the cross-wires, and the micrometer readings noted. The corresponding wave-lengths were then read off from the curve, and in this way, lists of the wave-lengths of the lines in the various spectra were compiled. These lists were then all thrown together into one table, giving the wave-lengths and intensities of all the lines recorded, and the spectra in which they appear.

For the wave-lengths thus obtained no greater accuracy than one indicated by four figures is claimed. It was my intention in the first instance to give five figures from the more elaborate tables of some of the elements given by other observers, but this had to be abandoned in consequence of the considerable variations found in the tables between the results as given by different observers.

First, as regards the gas from eliasite. The following list gives the lines obtained in the complete inquiry after the lines due to the old gases have been eliminated. It should be stated, however, that several of the lines have wave-lengths very near those of the old gases; these have been retained when the more intense lines of the old gases are absent from the spectra. These cases are pointed out in the table.

In the case of some of the lines in the visible part of the spectrum, more accurate wave-lengths have been recorded by means of a four prism Steinheil spectroscope. These lines are indicated by (*s*).

Attempts have been made to concentrate the eliasite gas by the process of sparking with oxygen over potash, but the quantity of gas remaining is so small, and so largely admixed with helium and argon, that a new research, using very much more material, is essential.

It should be remarked that the list of lines which have been observed and photographed in the spectrum of the gases from eliasite represents the results of several experiments which have been made with different samples of the mineral. Some of the lines have only been seen once, while others have been noted several times. This suggests that the origins of the lines are very diverse, and it seems probable that some constituents of the mixture of gases obtained are absorbed by the potash in the process of sparking.

Next, with regard to the other minerals already examined. As it is impossible for me to go on with this research for the next few months, it seems desirable, in the interest of other workers, to give at the same time a complete list of the unknown lines, so far as the observations have at present gone, indicating their mineral origins, and whether or not lines nearly coincident in position have been observed in any celestial body.

This information is given in the preceding table (pp. 136—139), in which for the sake of completeness the lines obtained from eliasite are also shown.

In most of the experimental work connected with this research, I have been assisted by Mr. Shackleton, while Mr. Fowler is generally responsible for the determination of wave-lengths in the less refrangible part of the spectrum, and Mr. Baxandall for the reduction of the photographs.

“The Relation between the Refraction of the Elements and their Chemical Equivalents.” By J. H. GLADSTONE, D.Sc., F.R.S. Received June 3, 1896.

This paper is intended to give a preliminary account of some recent investigations into the specific refraction of the elements. It may be conveniently divided into two distinct parts. The first part is a revision and extension of the list of specific and atomic refractions, which was first published in the ‘Phil. Trans.’ for 1870, and was reprinted with modifications in a lecture given at the Royal Institution in 1877. The second part is an amplification of some deductions made in that lecture.

PART I.—*The Specific and Atomic Refractions of the Elements.*

The following table contains the atomic weights, the specific refractions, and the atomic refractions of the elements as revised and extended. For the atomic weights I have adopted the numbers recently published by the American Chemical Society’s Committee on Atomic Weights (‘J. Amer. Chem. Soc.,’ vol. 17), revised up to January, 1894. In regard to the specific refraction, advantage has been taken of the work done in some departments of the inquiry by Landolt, Haagen, Brühl, Topsøe and Christiansen, Mascart, Becquerel, Kanonnikoff, Soret, Nasini, Ghira, Perrot, Tutton, Lord Rayleigh, Edwards, and others, as well as many additional observations made by myself or by Mr. Hibbert.

The atomic refraction is the product of the numbers in the two earlier columns, that is, it equals $P \frac{\mu-1}{d}$, where P is the atomic weight, and $\mu-1/d$ is the specific refraction, that is, the refractivity divided by the density. Of course, these are not generally determined by observations of the element itself, but are deduced from those of its salts or other compounds.

Element.	Atomic weight.	Specific refraction.	Atomic refraction.
Hydrogen	1·008	1·488	1·5
Lithium.....	7·0	0·514	3·6
Beryllium.....	9·0	0·733	6·6
Boron.....	11·0	0·436 or 0·317	4·8 or 3·5
Carbon.....	12·0	0·383	4·6
Nitrogen.....	14·03	0·343, &c.	4·8, &c.
Oxygen.....	16·0	0·203 or 0·169	3·25 or 2·7
Fluorine.....	19·0	0·031	0·6?
Argon.....	19·94	0·159	3·17
Sodium.....	23·05	0·202	4·65
Magnesium.....	24·3	0·287	6·9
Aluminium.....	27·0	0·352	9·5
Silicon.....	28·4	0·250 or 0·204	7·1 or 5·8
Phosphorus.....	31·0	0·594	18·4, &c.
Sulphur.....	32·0	0·422 or 0·500, &c.	13·5 or 16·0, &c.
Chlorine.....	35·45	0·282 or 0·302	10·0 or 10·7
Potassium.....	39·11	0·205	8·0
Calcium.....	40·0	0·252	10·1
Titanium.....	48·0	0·522	25·0
Vanadium.....	51·4	0·481	24·7?
Chromium.....	52·1	0·296	15·4
Manganese.....	55·0	0·208	11·5
Iron.....	56·0	0·209 or 0·355	11·7 or 19·9
Nickel.....	58·7	0·186	11·0
Cobalt.....	59·5	0·183	10·9
Copper.....	63·6	0·184	11·7
Zinc.....	65·3	0·151	9·9
Gallium.....	69·0	0·214	14·75
Arsenic.....	75·0	0·200	15·0
Selenium.....	79·0	0·339, &c.	26·8, &c.
Bromine.....	79·95	0·190 or 0·213	15·2 or 17·0
Rubidium.....	85·5	0·133	11·4
Strontium.....	87·66	0·152	13·3
Yttrium.....	89·1	0·197	17·6
Zirconium.....	90·6	0·242	21·9
Rhodium.....	103·0	0·232	23·9?
Palladium.....	106·5	0·213	22·7
Silver.....	107·92	0·121	13·1
Cadmium.....	112·0	0·124	13·9
Indium.....	113·7	0·153	17·4
Tin.....	119·0	0·232 or 0·161	27·6, or 19·2
Antimony.....	120·0	0·204 or 0·200	24·5 or 24·0
Iodine.....	126·85	0·192 or 0·214	24·4 or 27·2
Cæsium.....	132·9	0·117	15·6
Barium.....	137·43	0·117	16·1
Lanthanum.....	138·2	0·143	19·8
Cerium.....	140·2	0·143	20·0?
Iridium.....	193·1	0·165	31·9?
Platinum.....	195·0	0·172	33·5
Gold.....	197·3	0·127	25·1
Mercury.....	200·0	0·107 or 0·099	21·5 or 19·8?
Thallium.....	204·0	0·106	21·6
Lead.....	206·95	0·129 or 0·119	26·7? or 24·5
Bismuth.....	208·0	0·154	32·0?
Thorium.....	232·6	0·123	28·7

The most notable change from previous tables is the increase in the value of hydrogen and the decrease in that of carbon, but the necessity of this has been gradually recognised by the principal workers on the refraction of organic bodies. This in no way affects the well-determined value $\text{CH}_2 = 7.6$.

It should be borne in mind that the specific refraction cannot claim a constancy equal to that of the atomic weight. The latter is generally believed to be identical under all circumstances, though the element may be capable of combining with another in two or more multiple proportions. On the other hand, several of the elements, as oxygen and iron, exhibit two or more specific refractions, which are not in multiple proportion, but depend upon the manner of combination. The best recognised of these are given in the third column, and the existence of others is indicated by an "&c." Beside these well-marked differences, there are many smaller variations, scarcely, if at all, beyond the limits of experimental error, which depend upon differences of physical condition or chemical structure. The numbers given in column 3 are therefore subject to an uncertainty, which may in some instances amount to 5 per cent. Where there is a greater divergence among the values observed, or where the deductions have been made from only one specimen, it is indicated by a query.

PART II.—*The Relation between the Specific Refraction and the Combining Proportion of the Metals.*

In the paper "On the refraction equivalents of the elements" previously referred to, it was shown that if the metallic elements be arranged in the order of their specific refractions, they are roughly in the inverse order of their combining proportions.

In the lecture at the Royal Institution, I showed that this inverse order followed an approximate law, namely, that the "specific refractive energy of a metal is inversely as the square root of its combining proportion." This generalisation was proved for univalent metals, the figures showing (with the exception of sodium) a practically constant value for the product of the specific refraction and the square root of the combining proportion.

By the aid of the table in the first part of this communication, the generalisation can now be tested throughout the whole range of the metallic elements.

Univalent Metals.

Metal.	Specific refraction.	$\sqrt{\text{Combining proportion.}}$	Product.
Lithium.....	0·514	2·65	1·36
Sodium.....	0·202	4·80	0·97
Potassium.....	0·205	6·25	1·28
Rubidium.....	0·133	9·24	1·23
Silver.....	0·121	10·3	1·26
Cæsium.....	0·117	11·5	1·35
Mercury.....	0·107	14·1	1·51
Thallium.....	0·106	14·3	1·51

This confirms the conclusions drawn in 1877, the mean product for the univalent metals, omitting sodium, being 1·30. This is in spite of the fact that lithium and cæsium differ from one another in either factor in the ratio of about 9 to 2. The two metals below the line, though acting as monads in the compounds from which these values are deduced, are considered to be dyad and triad respectively. With them, the product is a little higher; this will be referred to afterwards.

Bivalent Metals.

Metal.	Specific refraction.	$\sqrt{\text{Combining proportion.}}$	Product.
Beryllium.....	0·733	2·12	1·55
Magnesium.....	0·287	3·49	1·00
Calcium.....	0·252	4·47	1·12
Zinc.....	0·151	5·71	0·87
Strontium.....	0·152	6·62	1·00
Cadmium.....	0·124	7·41	0·92
Barium.....	0·117	8·29	0·97
Mercury.....	0·099	10·0	0·99?
Copper.....	0·184	5·64	1·04
Cobalt.....	0·183	5·45	1·00
Nickel.....	0·186	5·42	1·01
Manganese.....	0·208	5·24	1·09
Iron.....	0·209	5·29	1·11
Lead.....	0·119	10·17	1·21
Tin.....	0·232	7·71	1·78
Palladium.....	0·213	7·30	1·55

With the exception of beryllium, those metals which are properly bivalent agree closely, although giving values distinctly below that of the former list. The mean of the values is 0·99. The remaining five metals, which have well-marked higher valencies, have, as in the case of the corresponding univalent elements, a somewhat higher value.

Trivalent Metals.

Metal.	Specific refraction.	$\sqrt{\text{Combining proportion.}}$	Product.
Aluminium.....	0·352	3·00	1·05
Gallium.....	0·214	4·79	1·02
Yttrium.....	0·197	5·45	1·07
Indium.....	0·153	6·15	0·94
Lanthanum.....	0·143	6·79	0·97
Cerium.....	0·143	6·83	0·98 ?
Gold.....	0·127	8·11	1·03
Arsenic.....	0·200	5·00	1·00
Antimony.....	0·204	6·32	1·29
Chromium.....	0·296	3·74	1·23
Iron.....	0·355	4·32	1·53

The trivalents proper and arsenic agree still more closely amongst themselves, and give a mean of 1·01, which is practically identical with that of the bivalents proper.

The other trivalents, which have well-marked higher valencies, exhibit, as before, a somewhat higher product.

Quadrivalent Metals.

Metal.	Specific refraction.	$\sqrt{\text{Combining proportion.}}$	Product.
Zirconium.....	0·242	4·76	1·15
Tin.....	0·161	5·45	0·88
Lead.....	0·129	7·19	1·02 ?
Thorium.....	0·123	7·62	0·94
Iridium.....	0·165	6·95	1·15 ?
Platinum.....	0·172	6·98	1·20

In this case the mean is 1·06, nearly the same as with the bivalents and trivalents, but the numbers are not so regular.

We have observations on one pentad, namely, antimony. This gives—

Specific refractive energy	0·200
$\sqrt{\text{Combining proportion}}$	4·9
Product	0·98

These tables show: First, that the metals which have the same valency, have the same, or nearly the same, constant of refraction for equivalent weights.

Secondly, that the constants of the bivalent, trivalent, quadrivalent, and apparently quinquivalent groups are practically the same, ranging about 1.01.

Thirdly, that when a metal combines in a proportion that indicates a lower valency than that ordinarily assigned to it, its constant is somewhat elevated.

I refrain at present from pointing out minor analogies between closely-allied metals, and from attempting to explain the difference between the univalent and the other groups; why sodium should fall away from the value proper to the alkaline group, and closely approximate to that of all the other groups; or why beryllium, bivalent tin, and trivalent iron should be represented by such exceptionally high figures.

It is to be understood that the values given are all deduced from compounds in which the metal plays the part of an electro-positive radicle. Where they combine with oxygen to form the electro-negative radicle, the values are completely altered, just as we find in the case of several non-metallic elements.

If we calculate these constants for the square root of the atomic weight instead of that of the combining proportion, we shall obtain for the means—

Univalents	1.30
Bivalents	1.40
Trivalents	1.75
Quadrivalents	2.12
Quinquivalent	2.19

This arrangement does not, as in the former case, give a practically identical constant for the bivalent, trivalent, quadrivalent, and quinquivalent metals. The fact that these numbers increase nearly in the proportion of the square roots of 2, 3, 4, and 5, indicates that the relation involved is not between the specific refraction and the atom, but rather between it and the combining proportion or chemical equivalent of the metal. This brings the optical property into analogy with Faraday's law of electro-chemical equivalents.

I propose to give this product the descriptive name, "Refractive constant of equivalent weights." It may be represented by—

$$SE^1 = \text{constant},$$

where S is the specific refraction, and E the chemical equivalent of the metal.

Some physicists may prefer to make use of the square of the above formula, namely,

$$S^2E = \text{constant}.$$

If the Lorenz expression for S , namely, $\frac{\mu^2 - 1}{\mu^2 + 2} \cdot \frac{1}{d}$, be preferred to $\mu - 1/d$, it may be substituted in either of the above formulæ.

In either case the actual numbers will, of course, be changed more or less, but the relation above pointed out will still hold good. The discrepancies will, however, be somewhat exaggerated by the change.

This is suggested as a first approximation to a new law. It may be useful in both chemical and physical science. It holds good, however, only for the metallic elements.

“Selective Absorption of Röntgen Rays.” By J. A. McCLELLAND, M.A., Fellow of the Royal University of Ireland. Communicated by Professor J. J. THOMSON, F.R.S. Received June 11,—Read June 18, 1896.

(From the Cavendish Laboratory.)

The experiments described in this paper were made to determine whether or not the Röntgen rays given off by a vacuum bulb were of a homogeneous nature, by examining the manner in which they are absorbed by different substances. The induction coil and vacuum bulb for producing the rays were enclosed in a wooden box thickly lined with metal, with a small hole in the top, directly beneath which and close up to it the vacuum bulb was placed. Over the hole a well-insulated metal disk was placed and connected to one pair of quadrants of an electrometer. The two pairs of quadrants are first connected together and with one terminal of a battery of small storage cells, the other terminal being connected to earth.

The quadrants of the electrometer are then separated from each other and from the storage cells, and the induction coil turned on. The Röntgen rays passing through the hole in the box and falling on the charged disk discharges it, and the intensity of the radiation is measured by the rate at which the spot of light from the electrometer needle moves across the scale. The metal lining of the box is connected to earth, and the small hole covered with a single sheet of tinfoil to screen the electrometer from direct electrical disturbances.

The substance whose absorptive power is to be examined is placed over the hole so that the rays traverse it before falling on the charged disk.

Evidences of selective absorption were sought for in the following manner. The rate of leakage was accurately determined when the rays were passing through one of the substances used, say a plate of glass. Sheets of tinfoil were then substituted for the glass and the

number— n , say—taken such that the leakage from the charged disk was approximately the same as when the glass was used. The rate of leak was then measured accurately. The ratio of the rate of leak with the glass to that with the n sheets of tinfoil gives a measure of their relative transparency to Röntgen rays.

A number of tinfoil sheets is now placed over the hole; the glass plate is placed on the top, and the rate of leak measured. The glass is removed and the same n sheets of tinfoil as were formerly used put in its place, and the leakage measured. The ratio of the rate of leak in the latter two cases is a measure of the relative transparency of the glass and the n tinfoil sheets to the Röntgen rays after they have been already screened by passing through several layers of tinfoil.

The two ratios thus obtained should be equal if the Röntgen rays are all of one kind, but if the glass is relatively less transparent in the second case it can only be explained by assuming that the Röntgen rays are not homogeneous, and that some of them are more readily absorbed by the glass and others by the tinfoil.

Various substances were tested against tinfoil in the manner described. With some there was no selective absorption, with others it was very marked. Glass gave none, with mica and paraffin the effect was small, with fuchsine, eosine, fluorescine, aesculin, and barium sulphide the effect was very marked. With several fluorescent screens the effect was also marked. Pure water also gave a distinct though smaller effect.

The table below sets forth the results obtained with these substances.

Column B gives the quotient of the rate of leak through the substance in column A to that through a number of tinfoil layers which gave approximately the same leak. Column C gives the quotient of the rate of leak through the substance to that through the same tinfoil layers after the rays have already passed through four layers of tinfoil.

A.	B.	C.	Difference.
Calcium tungstate.....	1.07	0.85	0.22
Calcium platinoeyanide	1.30	0.86	0.44
Luminous paint.....	1.0	0.71	0.29
Potassium platinoeyanide	1.10	0.87	0.23
Fuchsine.....	1.15	0.77	0.38
Eosine.....	1.31	1.00	0.31
Aesculin.....	1.33	0.90	0.43
Fluorescine.....	1.32	1.08	0.24
Barium sulphide.....	1.30	0.97	0.33

Of the substances used, the above showed the effect best, but with wood, paraffin, and water, although small, it could always be detected. We conclude from the above results that the Röntgen rays are of different kinds, and that the substances given in the table differ very much from tinfoil in their selective absorption. After the rays have been screened by passing through some tinfoil layers additional layers are much less absorbent, while the absorption produced by other substances is not so much diminished.

Of the substances tried, those which are fluorescent gave the most marked difference as compared with tinfoil.

The above results were all obtained with one vacuum tube, which was working extremely well. It produced a very rapid leak from the charged disk, and the pressure of its residual air was very small. In fact, after working for a time it became too strong for the coil that was being used to work it. Another vacuum tube, in which the pressure of the residual air was greater and which was not so efficient in producing leakages, was then used, and several of the substances used before were again tested, but in no case was any evidence of selective absorption obtained. As far as the test was efficient, the radiation from this bulb was homogeneous.

A third tube was then used, more efficient than the last in producing leakage, but not so good as the first used. With this tube experiments made in the same way as before gave evidence of selective absorption, but not so marked as with the first tube.

It seems therefore that as a tube becomes more efficient the character of the rays given off becomes less homogeneous.

“On the Structure of Metals, its Origin and Changes.” By F. OSMOND and W. C. ROBERTS-AUSTEN, F.R.S., Professor of Metallurgy, Royal College of Science. Received June 10, —Read June 18, 1896.

(Abstract.)

The authors begin their paper by stating that it has been shown by Herbert Tomlinson that the atomic volume of metals is intimately connected with their thermal capacity* and with Young's modulus.† He considers, in view of the work of Wertheim,‡ of Maxwell,§ and of Heen,|| and as the result of his own experiments, that the value of

* ‘Roy. Soc. Proc.’ vol. 38 (1884–85), p. 488.

† ‘Phil. Trans.’ Part I, 1883, p. 32.

‡ ‘Ann. de Chim. et de Phys.’ vol. 12, 1844.

§ ‘Phil. Trans.’ vol. 156, 1866, p. 249.

|| ‘Bull. de l'Acad. Roy. de Belgique,’ vol. 4 (1882).

the product of the elasticity E , when multiplied by a fractional power of the atomic volume $\frac{A}{D}$, is a constant for all metals,

$$E\left(\frac{A}{D}\right)^{\frac{2}{3}} = 181 \times 10^4.$$

The divergences shown by several metals from this mean value arise from the fact that the presence of small amounts of impurity makes a great difference in their elasticity.

Sutherland* finds a close relation between the atomic volume and the rigidity of metals, and considers that this rigidity is "in its essence a kinetic phenomenon, almost as simple in character as the elasticity of perfect gases."

Professor Fessenden,† moreover, has urged that the cohesion of metals is proportional to some power of the atomic volume, and he considers that the rigidity varies as the fifth power of the distance of the centre of the atoms, or as (atomic volume) ^{$\frac{2}{3}$} . It will be evident, therefore, that the atomic volume of a metal is very important.

One of the authors purified gold with great care, and alloyed seventeen separate portions of it with foreign elements in quantities which were in each case close to 0.2 per cent., and from each sample of this alloyed gold, bars were cast, 88 mm. long by 7.5 mm. wide by 5.2 mm. thick. The tensile strength, elongation, and reduction of sectional area (striction) were determined, and the results were published in the 'Phil. Trans.' in 1888. These results indicated in a general way, that the tenacity and ductility of gold is increased by the presence of 0.2 per cent. of an added element of smaller atomic volume than that of gold itself, while, on the other hand, these properties are diminished when the atomic volume of the added element is greater than that of gold.

There are, as might be expected, exceptions and irregularities, but it is strange that they are not more numerous and more marked. Even the purest metals are not, from a mechanical point of view, homogeneous. Under the influence of internal forces which tend to make them crystalline, and of external stresses which are set up by contraction during cooling, the invisible molecules become arranged in visible and more or less highly organised groups. These groups are separated from each other either by planes of cleavage or by joints which are often surfaces of least cohesion, and, therefore, of weakness. This is especially the case when these joints have been accentuated by the evolution of dissolved gas at the moment of the solidification of the metal. In alloys, chemical homogeneity may, in turn, disappear,

* 'Phil. Mag.,' vol. 32, 1891, p. 41.

† 'Chem. News,' vol. 66, 1892, p. 206.

and free metals, chemical compounds, or various alloys may fall out of solution from the liquid mass, and, finally, the *eutectic* alloy solidifies, but its presence, as a residual fluid facilitates the arrangement of the parts which have previously solidified.

One of the authors in collaboration with M. Werth* was probably the first to direct attention to the influence which these fusible residues, to which the name of "cements" was given, exert on the working of steel and on the mechanical properties of the finished products of steel manufacture. Since then M. André le Chatelier† has repeatedly insisted on this point, correctly enough as a principle, though perhaps with a tendency to generalise too much from ideas which are, in themselves, accurate.

It is possible to distinguish in metals and alloys both the *visible* structure and the *molecular* structure, and between them, such methods of investigation as it is possible to adopt, enable a well defined line of demarcation to be traced. Attention must, therefore, be directed to ascertaining to what extent the mechanical properties of a given sample of metal are due to each of these kinds of structure, and how far to their mutual relations. This being the case, the authors considered that it would be interesting to submit the gold, containing 0·2 per cent. of various elements, to micrographical examination, and, fortunately, the identical specimens which were submitted to the Royal Society, eight years ago, had been preserved intact, and were available for examination.

Descriptions are then given in detail of the methods adopted in preparing, polishing and etching the micro-sections of gold alloyed with various impurities, photographs of which sections illustrate the paper. It is difficult to give a brief abstract of the authors' conclusions, but they may be stated as follows. They consider it to be certain that there is no relation between either the structure, the appearance of the fractures, the melting points of the alloyed elements and the mechanical properties of the masses of alloyed gold. They observe that every iron metallurgist who examined the photograph of the micro-section of gold with potassium would form a highly favourable opinion as to the mechanical properties of the mass it represents, while it is really, from a mechanical point of view, the worst of the series. On the other hand he would think that the micro-section of the gold alloyed with zirconium, indicated a structure of deplorable weakness, while as a matter of fact it might equally well represent alloys which vary in tenacity from less than half a ton per square inch to $7\frac{1}{2}$ tons, and are either incapable of being extended, or will elongate 30 per cent.

The authors then proceed to examine the structure of the various sec-

* Osmond and Werth, 'Ann. des Mines,' vol. 8, 1885, p. 5.

† 'Inst. Mech. Engineers Proc.,' April, 1893, p. 191.

tions in detail, and they conclude the first part of the paper by stating that they do not contest in any way, as their previous publications abundantly prove, the importance of the part which may be played in the mechanical properties of the alloys by the residues which remain liquid after the main mass of the alloy has solidified, the alloys being tested either at the ordinary temperature or when heated. But, in order that it may be possible for such cements to intervene and affect the mechanical properties of alloys, the cements must at least have a real existence. Nothing indicates that they do exist in ten out of twelve of the alloys examined. The authors do not express themselves too positively on this point, for some new method of etching may reveal new facts. The impurities which are sought for may happen to concentrate themselves beyond the particular region which has been sectioned. These are, however, gratuitous suppositions. Polishing only indicates the presence of cement in two cases. The little secondary crystals which are described in the paper might readily be mistaken for cements, of definite or indefinite composition, if they were found only in certain specimens, and then in such proportions as could be accepted. But they occur everywhere, and in all cases with identical appearances, forms, and dimensions; and, moreover, are seen to be collected into crystallites which pervade the whole mass. These are, therefore, usually and indubitably due to the crystallization of gold itself, although the alloying substances sometimes (indium and probably potassium) join up the crystals in question. For the same reason the dark line of the joints, traced as furrows by the etching, are very rarely the empty tracks of cement which has been dissolved away by *aqua regia*; their formation, which it is easy to follow in all its phases, directly connects them with secondary crystallization. The authors are led to the belief that in the case of ten of their alloys of the gold with about 0.2 per cent. of various impurities, solidification of the whole mass has been directly accomplished at a single time, and that the foreign bodies have remained as *solidified* solutions, as they were *fluid* solutions when the alloys were liquid, the impurities being dissociated into their ions in both solid and liquid. Under these conditions it is difficult to invoke, as explaining the mechanical properties of the alloy, the intervention of hypothetical "cements" with relatively low fusing points.

In the second part of the paper attention is directed to the fact that gold alloyed with bismuth, thallium, antimony, and aluminium has its structure entirely changed by annealing it in sulphuric acid at about 250°. The large grains of the metal become divided into a multitude of little polyhedral grains. Nothing remains of the original structure, and the effect closely resembles that which is obtained by annealing steel castings at a bright red heat (800°). It is pointed out that whatever this observation may signify, the trans-

formation of the structure of a metal, at a temperature so far below its melting point, and, in the case of the gold-antimony and gold-aluminium series, far below the melting point of the *eutectic* alloys, in the presence of only two-tenths per cent. of a foreign body, is probably not an isolated fact, and appears to open a new field for research.

“On the Relations between the Viscosity (Internal Friction) of Liquids and their Chemical Nature. Part II.” By T. E. THORPE, LL.D., F.R.S., and J. W. RODGER, Assoc. R.C.S. Received May 27,—Read June 11, 1896.

(Abstract.)

In the Bakerian Lecture for 1894 the authors gave an account of their work on the viscosity of over seventy liquids, and they discussed the interdependence of viscosity and chemical composition. In order to render the investigation more complete, they have now made measurements of the viscosity of (1) a number of esters or ethereal salts, and (2) of ethers, simple and compound—groups of liquids, which with the exception of ethyl ether, have not hitherto been studied by them. The physicochemical relationships previously established made such determinations of special interest, for it was shown that one of the most striking of the various connexions traced between chemical constitution and viscosity was the influence exerted by oxygen according to the different modes in which it was assumed to be associated with other atoms in the molecule. The influence which could be ascribed to hydroxyl-oxygen differs to a most marked extent from that of carbonyl-oxygen, and it appeared that ether-oxygen, or oxygen linked to two carbon atoms, had also a value which differed considerably from oxygen in other conditions.

In the present paper the authors give the experimental values for the viscosity of the ten lowest fatty esters, carefully purified samples of which had been placed at their disposal by Professor Sydney Young. With the help of Mr. Barnett, B.Sc., Assoc. R.C.S., they have also investigated the viscosity of five fatty ethers. By the kindness of the Photometric Standards Committee they have also been enabled to make observations upon various samples of carefully prepared isopentane, and they have supplemented their former observations by a new series of experiments upon ethylbenzene, for a sample of which they are indebted to Dr. G. L. Moody, of the City and Guilds Central Institute.

The details of the observations are given in precisely the same manner as in the first paper, and formulæ of the Slotte type showing

the relation between viscosity in absolute measure and temperature are calculated for each liquid. The general results of the observations are then discussed in the same manner as in the previous memoir. With regard to the two hydrocarbons, it is found that the isopentane from fusel oil gives slightly different values from that originally observed, which was obtained from American petroleum, and which, although of an approximately constant boiling point, was undoubtedly a mixture. The new sample of ethylbenzene, however, gave results which were in very good agreement with those previously obtained.

The conclusions relating to the graphical representation of the results may be thus summarised. Both ethers and esters give no evidence of molecular aggregation, and conform to the rules that:—

- (1) In homologous series, the viscosity is greater the greater the molecular weight.
- (2) An iso-compound has a smaller viscosity than a normal isomer.
- (3) The more symmetrical the molecule of an isomeric compound the lower is the viscosity.

As regards the esters themselves, it is noteworthy, where the comparison is possible, that:—

- (4) Of isomeric esters, the formate has the larger viscosity.

As regards the algebraical representation of the results, it is shown that in the expression $\eta = C/(1 + \beta t + \gamma t^2)$, derived from Slotte's formula:—

- (1) In any homologous series, β and γ increase as the molecular weight increases.
- (2) Of isomeric compounds, the iso-compound has the smallest coefficient.
- (3) Ethyl ether, the symmetrical isomer, has smaller coefficients than methyl propyl ether.
- (4) As regards normal isomeric esters, the formate has the largest, and the propionate the smallest coefficients, and the values of the acetate are larger than of the butyrate.

The authors then deal with the relationships existing between the various viscosity magnitudes—the viscosity coefficient, the molecular viscosity, and the molecular viscosity work—(1) at the boiling point, and (2) at temperatures of equal slope, the slope adopted being that employed in their previous paper, namely, 0.0323, and values for the oxygen in three different conditions are given for each system of comparison in the same manner as in their first communication.

The two main results supported by all the methods of comparison, both at the boiling point and at temperatures of equal slope, are:—

- (1) That the effect which ether-oxygen exerts on the viscosity of a liquid differs to a marked extent from the effect exerted either by hydroxyl-oxygen or carbonyl-oxygen, and that
- (2) The viscosity of the formate is abnormally large when compared with that of other esters, and indicates that the exceptional behaviour of formic acid is to some extent retained by its ethereal salts.

“On the Determination of Freezing Points.” By J. A. HARKER, D.Sc. Communicated by Professor SCHUSTER, F.R.S. Received June 15,—Read June 18, 1896.

(Abstract.)

Of recent years great improvements have been made in the construction of accurate thermometers. For their graduation and study, the position of the thread for at least two fixed temperatures must be known with certainty, and one of these is generally the freezing point. According to many observers, the methods at present in use for the determination of this point are unsatisfactory and cannot be relied on, even when considerable precautions are taken, to more than about $0\cdot001^{\circ}$ to $0\cdot002^{\circ}$. The object of the present communication is to describe a method by which more consistent results can be obtained, and which is applicable to all kinds of thermometers.

The method adopted is to cool distilled water in a suitable vessel, protected from radiation, to a temperature below 0° , to insert the thermometer, and then bring about the freezing of the water by dropping in a crystal of ice. The thermometer then rises, and finally attains a steady temperature, differing only very slightly from the true zero.

Within the space allotted to this abstract, it is not possible to describe in detail all the precautions to be adopted and the apparatus employed, and for these reference must be made to the original paper. The following brief outline may, however, be given.

The apparatus consists of two portions, the thermostat and the cooler. The former is a rectangular copper vessel, filled with some liquid, which can be cooled below 0° without solidifying.

Generally either refined petroleum or a strong solution of common salt is employed. This vessel communicates by means of two wide tubes with a system of coils in the cooler, through which the liquid can be pumped by a rotary stirrer. These coils are surrounded by a freezing mixture at about -8° , and by this means the circulating liquid can be cooled and maintained for some time at about -2° . The distilled water to be frozen is contained in a tube of about 300 c.c. capacity made of clear glass. This is first placed directly

into the circulating liquid, and cooled quickly to -0.5° or -0.7° . It is then transferred to a copper cylinder lined with polished metal, placed in the centre of the thermostat, an annular space of about 1 cm. being left between them. The thermometer whose zero is to be taken is then quickly fixed in position in a spring clamp, the bulb and a considerable length of the stem above the zero being immersed in the water. A crystal of ice is dropped in, and the temperature quickly rises to the freezing point.

For the details of the arrangement for the illumination of the divisions, and taking the readings through the mass of the liquid containing the ice crystals in suspension, reference must be made to the paper.

The amount of ice formed in the liquid varies of course with the undercooling. Experiments made with good mercurial thermometers showed that if ice be present in sufficient quantity, the final temperature attained by the mixture of ice and water is not influenced perceptibly by variation of the temperature of the circulating liquid within fairly wide limits. As, however, it is extremely doubtful whether the indications of any mercurial thermometer can be relied on beyond 0.001° , it seemed desirable to control this result by some other means.

A platinum thermometer and bridge were therefore designed, capable of indicating with certainty a change of 0.0001° , and a description of the whole arrangement employed to attain this degree of accuracy forms the second half of the paper. The resistances in the bridge were of manganin, whose temperature coefficient is only about $\frac{1}{10}$ that of the usual resistance alloys, and the plugs usually employed for short circuiting the coils were replaced by copper bars and mercury contacts of specially low resistance. The thermometers employed were of about 10 ohms resistance, and were provided with the compensating leads, devised by Mr. Callendar. The maximum current which can be used in accurate measurements with these thermometers is about 0.02 ampère, and therefore the galvanometer employed required to be extremely sensitive. The instrument selected was a low resistance astatic one with vertical needle system of the type described by Weiss, and gives at the greatest sensibility at which the zero is steady one scale division for 1×10^{-10} ampère at 2500 scale divisions distance.

With this arrangement the influence of various conditions on the final temperature attained by the mixture of ice and water was studied. The results were found to be in close agreement with the theoretical deductions of Nernst, and it was found that with the right conditions, it was quite easy to keep the temperature in the freezing vessel constant, to within one or two ten-thousandths of a degree for an hour at a time.

The conclusion drawn from the previous experiments made with mercurial thermometers as to the small influence of changes in the external temperature, and in the temperature of the circulating liquid on that of the freezing vessel, was also confirmed, and it was found that in the final form of apparatus adopted, a change of two or three degrees in the temperature of the circulating liquid only caused the temperature of the mixture in the tube to alter by three or four ten-thousandths.

“Étude des Carbures Métalliques.” By M. HENRI MOISSAN.

Communicated by Professor RAMSAY, F.R.S. Received June 11,—Read June 18, 1896.

Les combinaisons définies et cristallisées du carbone avec les métalloïdes et les métaux étaient très peu connues jusqu'ici. On savait seulement que certains métaux tels que le fer, pouvaient dissoudre du carbone, et donner des fontes.

Les connaissances des chimistes sur ce point étaient peu étendues parce que ces combinaisons ne se produisent qu'à une température très élevée. L'application que j'ai faite de l'arc électrique comme moyen de chauffage d'un appareil de laboratoire m'a permis d'aborder cette question. Je résumerai mes recherches sur ce point dans cette note.

À la haute température du four électrique un certain nombre de métaux, tels que l'or, le bismuth, le plomb, et l'étain ne dissolvent pas de carbone.

Le cuivre liquide n'en prend qu'une très petite quantité, suffisante déjà pour changer ses propriétés et modifier profondément sa malléabilité.

L'argent à sa température d'ébullition dissout une petite quantité de carbone qu'il abandonne ensuite par refroidissement sous forme de graphite. Cette fonte d'argent, obtenue à très haute température, présente une propriété curieuse, celle d'augmenter de volume en passant de l'état liquide à l'état solide. Ce phénomène est analogue à celui que nous rencontrons dans le fer.

L'argent et le fer purs diminuent de volume en passant de l'état liquide à l'état solide. Au contraire, la fonte de fer et la fonte d'argent dans les mêmes circonstances augmenteront de volume.

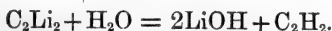
L'aluminium possède des propriétés identiques.

Les métaux du platine à leur température d'ébullition dissolvent le carbone avec facilité et l'abandonnent sous forme de graphite avant leur solidification. Ce graphite est foisonnant.

Un grand nombre de métaux vont, au contraire, à la température du four électrique produire des composés définis et cristallisés.

En 1836 Ed. Davy a démontré que le potassium pouvait s'unir au carbone et produire un corps décomposable par l'eau avec dégagement d'un nouveau carbure d'hydrogène. C'est ainsi que ce savant a découvert l'acétylène, dont la synthèse devait être réalisée plus tard par M. Berthelot.

En chauffant un mélange de lithine ou de carbonate de lithine et de charbon dans mon four électrique, j'ai pu obtenir avec facilité le carbure de lithium en cristaux transparents dégageant par kilogramme 487 litres de gaz acétylène pur.



De même en chauffant dans mon four électrique un mélange d'oxyde et de charbon, j'ai pu le premier obtenir par une méthode générale, à l'état pur et cristallisé et par notables quantités, les carbures de calcium, de baryum et de strontium. Le carbure de calcium avait été préparé auparavant à l'état de poudre noire amorphe et impure. Sans faire l'historique de la question je rappellerai les recherches de Wöhler, de M. Maquenne et celles de M. Travers sur ce sujet.

Tous ces carbures se détruisent au contact de l'eau froide avec dégagement d'acétylène. La réaction est complète, le gaz obtenu est absolument pur. Les trois carbures alcalino-terreux répondent à la formule C_2R , et le carbure de lithium à la formule C_2Li_2 . La préparation industrielle de l'acétylène est fondée sur cette réaction.

Un autre type de carbure cristallisé en lamelles hexagonales, transparentes, d'un centimètre de diamètre, nous est fourni par l'aluminium. Ce métal fortement chauffé au four électrique en présence de charbon se remplit de lamelles jaunes de carbure, que l'on peut isoler par un traitement assez délicat, au moyen d'une solution d'acide chlorhydrique étendu, refroidie à la température de la glace fondante.

Ce carbure métallique est décomposé par l'eau, à la température ordinaire, en fournissant de l'alumine et du gaz méthane pur. Il répond à la formule C_3Al_4 ,



Mon préparateur, M. Lebeau a obtenu dans les mêmes conditions le carbure de glucinium, qui lui aussi, fournit à froid avec l'eau un dégagement de méthane pur.

Les métaux de la cérîte vont nous donner des carbures cristallisés dont la formule sera semblable à celle des carbures alcalino-terreux C_2R .

Nous avons étudié spécialement, la décomposition par l'eau des carbures de cérium C_2Ce , de lanthane C_2La , d'yttrium C_2Y , et de thorium C_2Th .

Tous ces corps décomposent l'eau et fournissent un mélange gazeux,

riche en acétylène et contenant du méthane. Avec le carbure de thorium, l'acétylène diminue et le méthane augmente.

Toutes les expériences entreprises sur le fer ne nous ont jamais donné de composés définis et cristallisés. À la pression ordinaire et à haute température le fer n'a jamais fourni une combinaison définie.

On sait depuis longtemps, grâce aux recherches de MM. Troost et Hautefeuille, que le manganèse produit un carbure CMn_3 . Ce carbure peut être préparé avec le plus grande facilité au four électrique, et au contact de l'eau froide, il se décompose, en donnant un mélange à volumes égaux de méthane et d'hydrogène,



Le carbure d'uranium, C_3Ur_2 , que j'ai obtenu par les mêmes procédés, m'a présenté une réaction plus complexe; le carbure très bien cristallisé et transparent lorsqu'il est en lamelles très minces, se détruit au contact de l'eau et fournit un mélange gazeux qui contient une grande quantité de méthane, de l'hydrogène et de l'éthylène.

Mais le fait le plus intéressant présenté par ce carbure est le suivant. L'action de l'eau froide ne produit pas seulement des carbures gazeux. Il se forme en abondance des carbures liquides et solides. Les deux tiers du carbone de ce composé se retrouvent sous cette forme.

Les carbures de cérium et de lanthane par leur décomposition par l'eau nous ont fourni de même, bien qu'en quantité moindre, des carbures liquides et solides.

L'ensemble de ces carbures décomposable par l'eau à la température ordinaire, avec production d'hydrogènes carbonés, constitue une première classe de composés de la famille des carbures métalliques.

La deuxième classe sera fournie par des carbures ne décomposant pas l'eau à la température ordinaire tels que les carbures de molybdène, CMo_2 ; de tungstène, CW_2 ; de chrome, CCr_4 et C_2Cr_3 .

Ces derniers composés sont cristallisés non transparents, à reflets métalliques. Ils possèdent une grande dureté et ne fondent qu'à une température très élevée. Nous avons pu les préparer tous au four électrique et nous avons donné le détail de ces expériences ainsi que toutes les analyses aux 'Comptes rendus de l'Académie des Sciences de Paris.'

Les métalloïdes vont nous fournir aussi avec le carbone, à la température du four électrique, des composés cristallisés et définis. Nous citerons par exemple le carbure de silicium, CSi , découvert par M. Acheson, et préparé aujourd'hui dans l'industrie sous le nom de carborundum; le carbure de titane, CTi ; dont la dureté est assez grande pour permettre de tailler le diamant tendre; le carbure de zirconium, CZr ; le carbure de vanadium, CVa . Nous avons indiqué la préparation et les propriétés de ces nouveaux carbures.

Un fait général se dégage des nombreuses recherches que j'ai

entreprises au four électrique. Les composés qui se produisent à haute température sont toujours de formule très simple et le plus souvent il n'existe qu'une seule combinaison.

Mais la réaction qui nous a paru la plus curieuse dans ces recherches est la production facile de carbures d'hydrogène gazeux, liquides ou solides, par l'action de l'eau froide sur certains de ces carbures métalliques. Il nous a semblé que ces études pouvaient avoir quelque intérêt pour les géologues.

Les dégagements de méthane plus ou moins pur qui se rencontrent dans certains terrains, et qui durent depuis des siècles pourraient avoir pour origine l'action de l'eau sur le carbure d'aluminium.

En partant de quatre kilogrammes de carbure d'uranium, nous avons obtenu dans une seule expérience plus de 100 gr. de carbures liquides.

Le mélange ainsi obtenu est formé en grande partie de carbures éthyléniques non saturés, et en petite quantité de carbures acétyléniques. Ces carbures prennent naissance en présence d'une forte proportion de méthane et d'hydrogène à la pression et à la température ordinaire; ce qui nous amène à penser que lorsque la décomposition se fera à température élevée, il se produira des carbures saturés analogues aux pétroles.

M. Berthelot a établi en effet que la fixation directe de l'hydrogène sur un carbure non saturé pouvait être produite par l'action seule de la chaleur.

L'existence de ces nouveaux carbures métalliques destructibles par l'eau peuvent donc modifier les idées théoriques qui ont été données jusqu'ici pour expliquer la formation de quelques pétroles, ou autres produits carbonés. Il est bien certain que nous devons nous mettre en garde contre des généralisations trop hâtives.

Vraisemblablement il existe des pétroles d'origines différentes. A Autun, par exemple, les schistes bitumineux paraissent bien avoir été produits par la décomposition de matières organiques.

Au contraire, dans la Limagne, l'asphalte imprègne toutes les fissures du calcaire d'eau douce aquitainien, qui est bien pauvre en fossiles. Cette asphalte est en relation directe avec les filons de pépérite (tufs basaltiques), par conséquent en relation évidente avec les éruptions volcaniques de la Limagne.

Un sondage récent fait à Riom à 1200 mètres de profondeur a amené l'écoulement de quelques litres de pétrole. La formation de ce carbure liquide pourrait dans ce terrain être attribué à l'action de l'eau sur les carbures métalliques.

Nous avons démontré à propos du carbure de calcium dans quelles conditions ce composé peut se brûler et donner de l'acide carbonique. Il est vraisemblable que, dans les premières périodes géologiques de la terre, la presque totalité du carbone se trouvait sous

forme de carbures métalliques. Lorsque l'eau est intervenue dans les réactions les carbures métalliques ont donné des carbures d'hydrogène et par oxydation de l'acide carbonique.

On pourrait peut être trouver un exemple de cette réaction dans les environs de St. Nectaire. Les granits qui forment en cet endroit la bordure du bassin tertiaire laissent échapper d'une façon continue et en grande quantité du gaz acide carbonique.

Nous estimons aussi que certains phénomènes volcaniques pourraient être attribués à l'action de l'eau sur des carbures métalliques facilement décomposables. Tous les géologues savent que la dernière manifestation d'un centre volcanique consiste dans des émanations carburées très variées, allant de l'asphalte et du pétrole au terme ultime de toute oxydation, à l'acide carbonique.

Un mouvement du sol mettant en présence l'eau et les carbures métalliques peut produire un dégagement violent de masses gazeuses. En même temps que la température s'élève, les phénomènes de polymérisation des carbures interviennent pour fournir toute une série de produits complexes.

Les composés hydrogénés du carbone peuvent donc se former tout d'abord. Les phénomènes d'oxydation apparaissent ensuite et viennent compliquer les réactions. En certains endroits, une fissure volcanique peut agir comme une puissante cheminée d'appel. On sait que la nature des gaz recueillis dans les fumerolles varie suivant que l'appareil volcanique est immergé dans l'océan ou baigné par l'air atmosphérique. A Santorin, par exemple, M. Fouqué a recueilli de l'hydrogène libre dans les bouches volcaniques immergées, tandis qu'il n'a rencontré que de la vapeur d'eau dans les fissures aériennes.

L'existence de ces carbures métalliques si facile à préparer aux hautes températures, et qui vraisemblablement doivent se rencontrer dans les masses profondes du globe,* permettrait donc d'expliquer dans quelques cas la formation des carbures d'hydrogène liquides ou solides et la cause de certaines éruptions volcaniques.

“Complete Freezing-point Curves of Binary Alloys containing Silver or Copper, together with another Metal.” By C. T. HEYCOCK, M.A., F.R.S., and F. H. NEVILLE, M.A. Received June 6,—Read June 18, 1896.

(Abstract.)

The paper, of which the following is an abstract, contains the results of some experiments on the freezing points of alloys of two

* La différence entre la densité moyenne de la terre et celle de la couche superficielle semble indiquer l'existence d'une masse centrale riche en métal. La connaissance des météorites holosidères vient à l'appui de cette hypothèse.

metals, one of the two being in each case either silver or copper. It is an extension into temperatures as high as 1100°C. , of experiments similar to those at lower temperatures with which we have been occupied for the last seven years. The results of our previous experiments, in which mercury thermometers were used, are published in the 'Journal of the Chemical Society.' In the work described in this paper the determinations of temperature were made by means of platinum electrical resistance pyrometers of the Callendar-Griffiths type.

The paper is divided into four sections.

Section I contains a short survey of certain points in the theory of concentrated solutions which bear on the interpretation of the experiments.

Section II is devoted to an account of the experimental method.

Section III contains the results of the experiments in a tabular form, each table being followed by notes and remarks taken from the experimental note books.

Section IV contains the results expressed graphically as complete freezing-point curves, together with a discussion and a statement of the conclusions that can be arrived at from a study of each curve.

Section I.

If we plot the percentage composition of an alloy horizontally, and the freezing point vertically we get the freezing-point curve. This, for a pair of metals, would consist of two branches, each starting from the freezing point of a pure metal, and descending until they meet in the eutectic point. Our silver-copper curve gives a fair idea of this case.

If the metals A and B form a stable compound C, then the theory as developed by Bakhuis, Rooseboom, and by Le Chatelier makes it probable that the curve will be divided into the systems A C and C B with two eutectic points, and an intermediate summit at C. This case is well illustrated by a complete freezing-point curve of copper-antimony by Professor Le Chatelier, in which two such summits occur.

Another not infrequent case is probably that of a compound, which when molten can only exist in a partially dissociated condition. Our silver-antimony curve resembles such a curve. Other points of Section I will be best deferred to the summary of Section IV.

Section II.

The alloys, weighing from 200 to 500 grams, were melted in plumbago (salamander) crucibles, placed in one of Fletcher's blast furnaces.

A current of coal gas or of hydrogen was passed through a pipe-stem into the crucible; and this gas, burning over the surface of the molten metal, proved a perfect protection against oxidation. The metal was stirred by a plunging stirrer of graphite. The alloys were made by adding weighed quantities of the second metal in succession to what was originally a weighed quantity of the first metal, and taking the freezing point after each addition.

Section III.

This section contains tables divided into parts and into series. The tables give the freezing point and the composition of each alloy, expressed in percentage weights of one of the constituent metals, and also in atomic percentages. By atomic percentage we understand the number of atomic weights of one metal contained in every 100 atomic weights of the two metals in the alloy.

Section IV.

The complete freezing-point curves given in the paper are for the following pairs of metals—Ag-Cu, Ag-Pb, Ag-Sn, Pb-Cu, Sn-Cu, Ag-Sb. But incomplete curves are also given, showing the freezing points of dilute solutions of Bi, Au, Ni, Fe, Al, in copper, and of Bi, Pt, Au, Al, and Tl, in silver.

It has not been our aim to make a special study of very dilute solutions, but the results we have obtained when utilised in the equations given in the paper give as the latent heat of fusion of a gram of copper the number 50 calories, and as the corresponding latent heat of silver 27 calories. This latter number is considerably greater than the 21 calories given by Person, and both numbers can only be regarded as provisional.

The silver-copper curve shows no indication of chemical combination, unless it be the unexpected fact that the eutectic alloy occurs exactly at the composition Ag_3Cu_2 . The comparatively small value of the two atomic falls makes it improbable that the two metals form monatomic molecules in this alloy.

In the silver-lead and silver-tin curves, which have a good deal of likeness to each other, the eutectic alloy contains so little silver that the curve consists almost wholly of the branch starting from pure silver. For the first 20 atoms of added metal the lead curve agrees very well, and the tin curve fairly, with the ideal curve of equation (2); but with more lead or tin the total depression becomes much less than that of the ideal curve at the same concentration. We are disposed to see in this, not an evidence of chemical combination, but rather an aggregation of the lead or tin atoms into larger

molecules, a process which, in the case of the silver-lead, might almost amount to the separation of the alloy into conjugate liquids near 50 atomic percentages of lead.

The lead-copper affords an excellent example of a phenomenon which has been predicted, we believe, by Ostwald, but, so far as we know, has not hitherto been examined experimentally. It is that of the solidification of a system consisting of two conjugate liquids, a saturated solution of lead in copper, and a saturated solution of copper in lead. For dilute solutions of lead in copper, as far as 7 atoms of lead, the curve is in harmony with equation (2); but as more lead is added its effect rapidly decreases, and from 17 to 65 atoms of lead the freezing point remains constant at 954° C. With more lead the freezing point again falls, until it reaches the eutectic point. An examination of the solid alloys shows that the flat part of the curve corresponds to alloys which have separated into two layers, while still liquid.

The copper-tin curve embraces all the remarkable bronzes, gun metal, bell metal, speculum metal, and it is not surprising to find that it presents singularities. The rapid increase in the steepness of the curve as tin is added suggests that the tin is combining with the copper to form complex molecules, perhaps of SnCu_3 or SnCu_4 , which exist in solution. An abrupt change, not only in the direction of the curve, but also in the character of the freezing point, and the nature of the precipitate at 15.2 atoms of tin is in accordance with the great changes in the physical and microscopical character of the alloy noted by Behrens as occurring here. The remarkably straight line of freezing points from here up to 20 atoms of tin is best explained on the assumption that an isomorphous mixture of SnCu_4 and another body are separating. The very flat part of the curve between 20 and 25 atoms of tin, along which each freezing point is an extremely constant temperature may be due to another case of isomorphism, or may be due to the separation of conjugate liquids. The existence of a body SnCu_3 is not clearly indicated by our curve, although not inconsistent with it. Double freezing points occur on the horizontal lines stretching to the left from 15.2 and 20 atoms of tin.

The silver-antimony curve shows an angle at Ag_3Sb , but the eutectic point, though near Ag_3Sb_2 , is not at this formula.

It is worthy of note that in three cases in our curves an angular depression, and not a summit, occurs at a formula point.

We have made a few experiments on alloys of gold, nickel, and iron, in copper. The two latter cause a rise, but gold produces a fall in the freezing point.

From what we have hitherto done, silver bismuth promises to resemble silver-antimony, copper-bismuth to resemble copper-lead. The silver-gold curve, as is already known, rises above the freezing

point of silver; and the same is true of silver-platinum. The silver aluminium curve presents some singularities; but here, as with other aluminium alloys, we have been troubled by partial oxidation of the aluminium, and we therefore hope to revise our experiments with this metal, before publishing them in full.

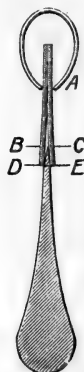
“Note of the Radius of Curvature of a Cutting Edge.” By A. MALLOCK. Communicated by LORD KELVIN, F.R.S.
Received June 9,—Read June 18, 1896.

The following note may be of interest, partly as indicating the extreme thinness to which a cutting edge may be brought by the ordinary process of grinding, and partly also as showing how readily the wave-length of light may be used, with only the simplest appliances, as a practical unit for the measurement of small distances.

The object in view was to find the thickness, or at any rate a superior limit to the thickness, of the cutting edge of a razor, and for this purpose two pieces of thin glass (such as is used for covering microscope slides) were prepared about $\frac{1}{2}$ inch long and $\frac{1}{16}$ wide.

These were pressed together by a small steel clip A, and the edge of the razor was inserted between them as shown in fig. (1).

FIG. 1.



The razor with the thin glasses in this position was then placed on the micrometer stage of a microscope and illuminated perpendicularly with light from a soda flame.

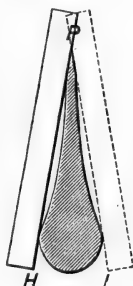
With the microscope, interference bands were of course visible between the thin glasses; and the number of bands, (N), counting from the spot where the clip pressed the glasses into optical contact

up to the edge of the blade, gives the distance BC in terms of the half wave-length.

The distance, BD, was measured by the micrometer, and the number obtained by dividing BD by the distance between the consecutive bands in the neighbourhood of the edge (since the thin glasses are hardly at all bent so near their free ends) gives when added to N the number of half wave-lengths in DE.

After these measures had been made, a piece of flat glass was laid on the blade of the razor as in fig. (2) and the number of interference

FIG. 2.



bands which appeared between the edge and a line parallel to the edge, but distant BD from it, was counted; and this observation was repeated with the flat glass on the opposite side of the blade.

The angle H P I, *i.e.*, the angle between the two positions of the flat glass, was also measured.

If the grinding of the razor was perfect and there was no rounding at the edge, no interference bands would be visible between the blade and the glass, but the two would be in contact up to the actual edge.

Fig. (3) is a large scale cross-section of the blade in the neighbourhood of the edge.

The thickness of the edge K L is

$$D'E' - (D'F + GE') - (FK + LG).$$

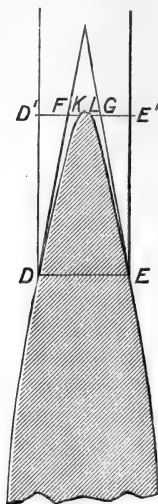
If we put N = number of bands between the clip A and the razor edge;

e = the distance between consecutive bands near the edge;

$\left. \begin{matrix} n_1 \\ n_2 \end{matrix} \right\}$ = the number of bands between the flat glass and either side of the blade in a distance DB from the edge;

$$BD = a \quad \text{and} \quad HPI = \theta.$$

FIG. 3.



We have

$$D'E' = DE = \frac{1}{2}\lambda(N + a/e);$$

$$D'F + GE' = 2a \sin \frac{1}{2}\theta, \text{ nearly};$$

$$FK + LG = (n_1 + n_2)\frac{1}{2}\lambda.$$

Whence

$$KL = \frac{1}{2}\lambda \left(N - n_1 - n_2 + \frac{a}{e} \right) - 2a \sin \frac{1}{2}\theta.$$

In the case of the razor on which these measures were made $N = 85$ $n_1 = 3$ $n_2 = 2$ $a/e = 8a = 0.00405$ in. $\sin \frac{1}{2}\theta = \frac{1}{8}$ and since for soda light $\frac{1}{2}\lambda = 0.0000116$ in. nearly

$$KL = 0.0000116 \times 88 - 0.00405 \times 0.25$$

$$= 0.00102 - 0.00101, \text{ nearly.}$$

Thus KL is not greater than 0.00001 , and if it is assumed that the actual edge has the curved cross-section, indicated by the dotted line in fig. 3, the radius of curvature cannot be greater than $1/200,000$ of an inch.

A well sharpened razor will cut a hair, when merely pressed against it at about an eighth of an inch, or rather more, from the place where the hair is held.

Human hair taken from the head has a circular cross-section, and varies in diameter in different individuals from 0.002 to 0.004 in.

With a hair of 0.0025 in. diameter, fixed at one end and free at the other, it was found that half a grain acting at an eighth of an inch from the fixed end, bent it through an angle of about 30° .

The razor applied at the same distance from the fixed end would sometimes cut through the hair before it had bent it as much as 30° ; and this shows that a force of half a grain must make the pressure per unit area at the place of contact sufficient to cause crushing or disruption of the material even when the edge has entered the hair to a distance comparable with the radius of the latter.

If we assume that the thickness of the edge is $1/100,000$ in. and that it has entered the hair until the length of the edge engaged is $1/1,000$ in., the area in contact will be about $1/100,000,000$ of a square inch and the pressure per square inch rather more than 3 tons, if the total force over the area of contact is half a grain.

It is difficult to get any direct measure of the pressure required to destroy by crushing or shearing the material of which hair is composed, but horn which is of the same nature requires a much larger pressure than 3 tons per square inch to crush it.

A rough experiment showed that a cylindrical steel punch with a flat end, began to sink into a block of horn when the pressure was between 12 and 16 tons per square inch.

It would seem, therefore, that although the optical method shows that the thickness at the edge cannot be greater than $1/100,000$ inch, the real thickness judged by the pressure per unit area necessary to cause the edge to cut in the way it actually does, must be considerably less than this.

PRS 60

“On the Determination of the Wave-length of Electric Radiation by Diffraction Grating.” By JAGADIS CHUNDER BOSE, M.A. (Cantab.), D.Sc. (Lond.), Professor of Physical Science, Presidency College, Calcutta. Communicated by LORD RAYLEIGH, Sec. R.S. Received June 2,—Read June 18, 1896.

While engaged in the determination of the “Indices of Refraction of various Substances for the Electric Ray” (*vide* ‘Proceedings of the Royal Society,’ vol. 59, p. 160), it seemed to me that the results obtained would be rendered more definite if the wave-length of the radiation could at the same time be specified. Assuming the relation between the dielectric constant K and the index μ as indicated by Maxwell, to hold good in all cases, it would follow that the index could be deduced from the dielectric constant and *vice versa*. The values of K found for the same substance by different observers are, however, found not to agree very well with each other. This may, to a certain extent, be due to the different rates of alternation of the field to which the dielectrics were subjected. It has been found in general that the value of K is higher for slower rates of alternation

and the deduced value of μ would therefore be higher for slow oscillations, the longer waves being thus the more refrangible. The order of refrangibilities would in such a case appear to be somewhat analogous to that in an anomalously dispersive medium like iodine vapour.

With exceedingly quick ethereal vibrations which give rise to light, there is an inversion of the above state of things, *i.e.*, the shorter waves are generally found to be the more refrangible. It would thus appear that there is a neutral vibration region for each substance at which this inversion takes place, and where a transparent medium produces no dispersion.

It would be interesting to be able to determine the indices of refraction corresponding to different wave lengths, chosen as widely apart as possible, and plot a curve of refrangibilities. A curve could thus be obtained for rock salt, which is very transparent to luminous and obscure radiations, and fairly so to electric radiation. Carbon bisulphide, which is very transparent to all but the ultra-violet radiation, would also be a good substance for experiment.

For the construction of a curve of refrangibility for electric rays, having different vibration frequencies, the indices could be determined by the method of total reflection referred to above. The determination of the corresponding wave-lengths, however, offers great difficulties. Hertz used for this purpose the method of interference, the positions of nodes and loops of stationary undulation produced by perpendicular reflection being determined by means of tuned circular resonators.

Sarasin and De la Rive subsequently repeated these experiments with different sized vibrators and resonators. They found that the apparent wave-length depended solely on the size of the resonators. The wave-length found was approximately equal to eight times the diameter of the circular resonator. From these experiments it was supposed that the radiator emitted a continuous spectrum consisting of waves of different lengths, and that the different receivers simply resonated to vibrations with which they happened to be in tune. If this supposition be true the emitted radiation should, by the action of a prism, or better still, a diffraction grating, spread out in the form of a continuous spectrum. If, on the contrary, the radiation is monochromatic, the spectrum should be linear. The experiments to be described below may throw some light on this question.

Professor J. J. Thomson, referring to the above case, is of opinion that the hypothesis of a continuous spectrum is highly improbable. It is more likely that, owing to the oscillation being of a dead-beat character, the resonator is set in vibration by the impact of incident electric waves. Each resonator vibrating at its particular free period,

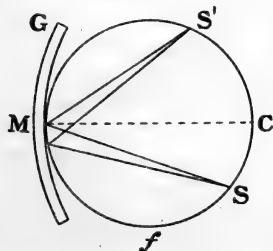
measures its own wave-length. There is, however, one difficulty in reconciling the theoretical value with that actually obtained. According to theory, the wave-length should be equal to twice the circumference, or 2π times the diameter of the circular resonator. The value actually obtained by Messrs. Sarasin and De la Rive is, as has been said before, eight times the diameter of the circle.

Rubens, using a bolometer and Lecher's modification of the slide bridge, determined the nodes and loops in a secondary circuit in which stationary electric waves were produced. A curve obtained by representing the bolometer deflections as ordinates and the distances of the bridge from one end as abscissæ, shows the harmonic character of the electric disturbance in the wire. It was found that the wave-length obtained by this method did not depend on the period of the primary vibrator; the wave-length measured was merely that of the free vibration started *in the secondary circuit* by the primary disturbance.

Hertz's method is therefore the only one for the measurement of electric waves in air, and the result obtained by this method is vitiated by the influence of the periodicity of the resonator. It was therefore thought desirable to obtain the wave-length of electric radiation in free space by a method unaffected by any peculiarity of the receiver.

I have succeeded in determining the wave-length of electric radiation by the use of curved gratings, and the results obtained seem to be possessed of considerable degrees of accuracy. Rowland's method of using the curved grating for obtaining diffraction light spectra was also found well suited for the production of pure spectra of electric radiation. The focal curve f in this arrangement is a circle, having as a diameter the straight line joining the centre of curvature C with the apex M of the grating.

FIG. 1.



G , the grating; M , its apex; f , the focal curve.

A source of radiation situated on this curve will give a diffracted spectrum, situated on the same curve defined by the equation

$$(a+b) (\sin i \pm \sin \theta) = n\lambda,$$

where $a+b$ is the sum of breadths of strip and space in the grating, i = angle of incidence, θ = angle of diffraction. The sign of θ is taken positive when it lies on the same side of the normal as the incident radiation.

In the above equation there are two interesting cases :—

- (1) When the receiver is placed at C, $\theta = 0^\circ$

$$(a+b) \sin i = n\lambda.$$

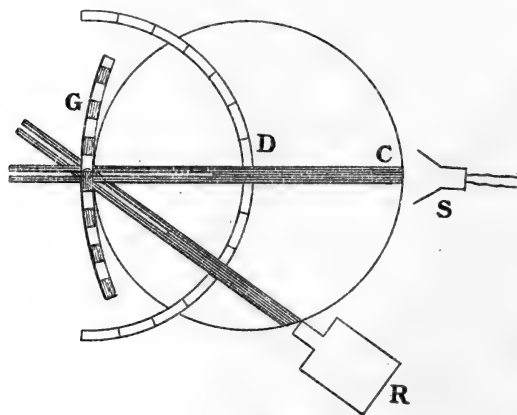
- (2) When the deviation is minimum $i = \theta$

$$2(a+b) \sin i = n\lambda.$$

Arrangement of the Apparatus.

The grating, which is cylindrical, is placed vertically on a wooden table, with its centre at C, occupied in the diagram by the spiral spring coherer S. With the radius, which joins the centre to the apex of the grating, as a diameter, a circle is engraved on the table—the focal curve—on which the radiator and the receiver are always kept. A pin is fixed immediately below the apex, and a graduated ring sunk in the table with this pin as the centre. The graduated

FIG. 2.



The radiator, R, and the receiver, S, revolve round a pivot vertically below the apex of the grating, along the focal curve. The angles are measured by the graduated circle, D.

circle is used for the measurement of the angles of incidence and diffraction. Two radial arms revolving round the pin carry the radiator and the receiver. The ends of the arms near the pin have

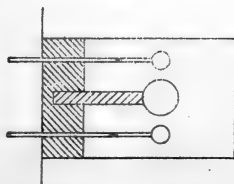
narrow slits, through which the pin projects. The slits allow the necessary sliding for placing the radiator and the receiver on the focal curve. It would be better to have the sliding arrangement at the free ends of the arms, the pin passing through the central ends, acting as a pivot. The circle is graduated into degrees, but one-fourth of a degree may be estimated.

Description of the Apparatus.

The Radiator.—Electric oscillation is produced between two metallic beads and an interposed sphere 0.78 cm. in diameter. The beads and the interposed sphere were at first thickly coated with gold, and the surface highly polished. This worked satisfactorily for a time, but, after long-continued action, the surface of the ball became roughened, and the discharge ceased to be oscillatory. After some difficulty in obtaining the requisite high temperature, I succeeded in casting a solid ball and two beads of platinum. There is now no difficulty in obtaining an oscillatory discharge, and the ball does not require so much looking after.

As an electric generator, I at first used a small Ruhmkorff's coil, actuated by a battery. I, however, soon found that the usual vibrating arrangement is a source of trouble; the contact points soon get worn out, and the break becomes irregular. The oscillation produced by a single break is quite sufficient for a single experiment, and it is a mere waste to have a series of useless oscillations. But the most serious objection to the continuous production of secondary sparks is the deteriorating action on the spark balls. Anyone who has tried to obtain an oscillatory discharge knows how easily the discharge becomes irregular, and the most fruitful source of trouble is often traced to the disintegration of the sparking surface. In my later apparatus I have discarded the use of the vibrating interrupter. The coil has also been somewhat modified. A long strip of paraffined paper is taken, and tinfoil pasted on opposite sides; this long roll is wound round the secondary to act as a condenser, and appropriate connexions made with the interrupting key. This arrangement

FIG. 3.

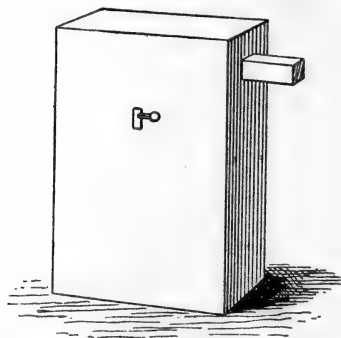


The Radiator.

secures a great saving of space. Two jointed electrodes carry the two beads at their ends; the distance between the beads and the interposed ball can be thus adjusted. This is a matter of importance, as the receiver does not properly respond when the spark-length is too large. Small sparks are found more effective with the receiver used. After a little experience it is possible to tell whether the discharge is oscillatory or not. The effective sparks have a smooth sound, whereas non-oscillatory discharges give rise to a peculiar cracked sound, and appear jagged in outline.

The wires of the primary coil are in connexion with a small storage cell through a tapping key. The coil, a small storage cell, and the key are enclosed in a tinned iron box. It must be borne in mind that a magnetic disturbance is produced each time the primary

FIG. 4.



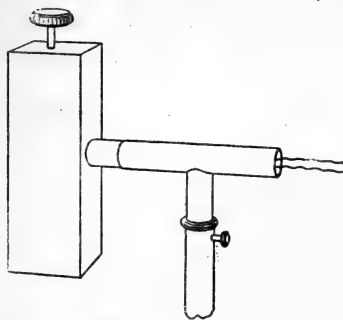
The Radiating Box, one-fifth natural size.

circuit of the induction coil is made or broken; a sudden variation of the magnetic field disturbs the receiver. The iron box in which the coil is enclosed screens the space outside from magnetic disturbance. On one side of the box there is a narrow slit through which the stud of the press-key projects. In front of the box is the radiator tube, which may be square or cylindrical. The radiating apparatus used in the following experiments has a square tube 1 sq. in. in section. The apparatus thus constructed is very portable. The one which I often use is 7 in. in height, 6 in. in length, and 4 in. in breadth. To obtain a flash of radiation it is merely necessary to press the key and then release it. The break is made very sudden by an elastic spring.

The Spiral Spring Receiver.—The receiving circuit consists of a spiral spring coherer in series with a voltaic cell and a dead-beat galvanometer of D'Arsonval type. An account of this form of receiver has already been given (*vide* "On the Indices of Refraction of

various Substances for the Electric Ray," 'Roy. Soc. Proc.,' vol. 59, p. 163). The receiver is made linear by arranging bits of steel spiral springs side by side, the sensitive surface being 3 mm. broad and 2 cm. in length. An electrical current enters along the breadth of the top spiral and leaves by the lowest spiral, having to traverse the intermediate spirals along the numerous points of contact. The resistance of the receiving circuit is thus almost entirely concentrated

FIG. 5.

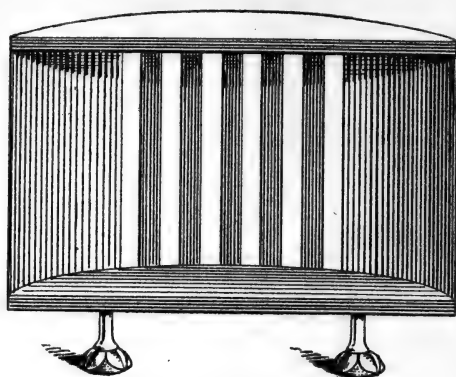


The Spiral Spring Coherer.

at the sensitive contact surface, there being little useless short circuiting by the mass of the conducting layer. When electric radiation is absorbed by the sensitive surface, there is a sudden diminution of the resistance, and the galvanometer in circuit is violently deflected. By adjusting the electromotive force of the circuit the sensitiveness of the receiver may be increased to any extent desirable. The receiver at each particular adjustment responds best to a definite range of vibration lying within about an octave. The same receiver could, however, be made to respond to a different range by an appropriate change of the electromotive force acting on the circuit. Very careful adjustment of the E.M.F. of the circuit is necessary to make the receiver respond at its best to a particular range of electric vibration.

The Cylindrical Grating.—The source of radiation—the spark gap—being a line, the curved diffraction grating is made cylindrical. The spark gap is always kept vertical; the grating is made of equidistant metallic strips, which are vertical and parallel. A piece of thin sheet ebonite is bent in the shape of a portion of a cylinder and kept in that shape by screwing against upper and lower circular guide pieces of wood. Against the concave side of the ebonite are stuck strips of rather thick tinfoil at equal intervals. Five different

FIG. 6.



The Cylindrical Diffraction Grating.

gratings were thus made with strips or spaces equal to 3 cm., 2.5 cm., 2 cm., 1.5 cm., and 1 cm. respectively.

The diameter of the cylindrical grating is 100 cm. It would perhaps have been better to use a grating with a less curvature, but it must be remembered that the intensity of radiation is very feeble, and I was apprehensive of the receiver failing to respond when placed at too great a distance. I find from the sensibility of the receiver used that it would be possible to increase the diameter of the cylinder to about 150 cm., and this size I intend to use in the construction of my next grating. The aperture of the grating is in the following experiments reduced to the smallest practicable limit.

Account of the Experiments.

The receiver being placed at a suitable position on the focal curve, the radiator is moved about on the same curve till the diffracted image falling on the receiver produces response in the galvanometer. The procedure adopted is as follows. The receiver is placed, say, at the centre of the grating ($\theta = 0^\circ$). The electric ray at first falls on the grating at a large angle of incidence. A series of flashes of electric radiation are now produced by manipulating the key, and the angle of incidence gradually decreased till the receiver suddenly responds. The angle of incidence corresponding to the zero angle of diffraction is thus determined. The receiver is then placed at a new position on the focal curve, and the corresponding angle of incidence determined as before. In this way a series of angles of incidence, with their corresponding angles of diffraction, are found for each grating.

It should be remarked here that numerous difficulties were encountered in carrying out the experiments. The reflections from the walls of the room, from the table, &c., were at first sources of considerable trouble. By taking special care, I succeeded in eliminating these disturbances. The radiating balls were placed about 1 cm. inside the square tube. This prevented the lateral waves acting on the receiver. The receiver was provided with a guard tube, which stopped all but the diffracted radiation reaching the sensitive surface. The insulated wires from the ends of the receiver were protected by thick coatings of tinfoil, and led to the galvanometer, which was placed at a considerable distance. The cell and the galvanometer were enclosed in a metallic case with a narrow slit for the passage of light reflected from the galvanometer.

In spite of all these precautions, I was baffled for more than six months by some unknown cause of disturbance which I could not for a long time account for. It was only recently, when nearly convinced of the futility of further perseverance, that I discovered the mistake in supposing sheets of tinned iron to be perfectly opaque to electric radiation. The metal box which contains the radiating apparatus seems to transmit a small amount of radiation through its walls, and if the receiver happens to be in a very sensitive condition it responds to the feeble transmitted radiation. I then made a second metallic cover for the radiating box, which precaution was found effective, provided the receiver was not brought very close to the radiator. The receiver is still affected if placed *immediately above* the radiator tube, though two metallic sheets be intervening. For this reason I had to postpone taking the reading for minimum deviation till I had made a radiation-proof box. A soft iron box (to prevent escape of magnetic lines of induction); enclosed in a second enclosure of thick copper, would, I expect, be found impervious to electric radiation.

With the second protective enclosure, all difficulties were practically removed. As a test for the absence of all disturbing causes, I observed whether the receiver remained unaffected when the grating was "off." There is a further test for the absence of external disturbances. The response, if only due to the diffracted beam, depends on the position of the radiator on the focal curve. If this angle of incidence is decreased, there should then be no action on the receiver. I found the positions of the radiator on the focal curve producing action on the receiver, to be well defined, and I experienced no further disturbance due to stray radiations.

The grating is fixed vertically on the table, so that its centre is at the same height as that of the middle of the receiving and radiating tubes. A small mirror is fixed at the middle of the central strip. The observer, placing his eye at the same height as that of the

radiator, levels the grating till the image of the eye is seen reflected by the mirror.

I first obtained an approximate value of the wave-length with a 2-cm. grating, and then took careful and systematic readings with the different gratings. By different gratings is meant the same curved piece of ebonite, on which strips of different breadths were successively applied. The grating was found fairly adjusted, and the readings taken on the right side of the grating agreed well with the corresponding ones on the left side. I did not, therefore, think it necessary to take double readings, but took the various readings alternately on the right and on the left side. In one case only I found the grating on one side giving slightly better reading than the other. When the incident angle is too oblique, the diffracted image is not sharp, and I therefore did not extend the reading beyond 40° of incidence. Spectra of the first order only were observed. The response in the receiving circuit was somewhat feeble when 1 cm. or 1.5 cm. grating was used. But a 2-cm. grating gave stronger indications. With 2.5 and 3 cm. gratings the response was very energetic and the definition of the diffracted spectrum very sharp. For example, when the receiver was kept fixed, and the angle of incidence gradually varied, there was an abrupt and strong response produced in the receiving circuit, as soon as the angle of incidence attained the proper value. A slight variation of this angle, even of less than a quarter of a degree, produced displacement of the diffracted image, and there was then no further action on the receiver. Had my graduated circle permitted it, I could have got more accurate readings. The radial arms carrying the receiver and radiator were of too primitive a design to make it worth while to attempt greater accuracy. I give below the readings of the angles of incidence and the corresponding angles of diffraction obtained with the different gratings, and the wave-length deduced from them.

Grating A.—Breadth of strip = 1 cm.

$i.$	$\theta.$	$\lambda.$	Mean λ for A.
38.0°	18°	1.849	1.843
35.0	20	1.831	
37.0	19	1.854	
38.75	17	1.837	

Grating B.—Breadth of strip = 1.5 cm.

<i>i.</i>	θ .	λ .	Mean for B.
38.0°	0°	1.847	1.844
26.0	10	1.836	
28.5	8	1.849	

Grating C.—Breadth of strip = 2 cm.

<i>i.</i>	θ .	λ .	Mean for C.
27.5°	0°	1.846	1.849
22.0	5	1.847	
20.0	7	1.855	

Grating D.—Breadth of strip = 2.5 cm.

<i>i.</i>	θ .	λ .	Mean for D.
21.5°	0°	1.832	1.845
29.5	— 7	1.852	
33.0	— 10	1.854	
34.0	— 11	1.841	

Grating E.—Breadth of strip = 3 cm.

<i>i.</i>	θ .	λ .	Mean for E.
18.0°	0°	1.854	1.848
23.25	— 5	1.845	
25.5	— 7	1.851	
31.0	— 12	1.843	

It would thus be seen that the different values of wave-length obtained from the above experiments are concordant, the mean value being 1.846 cm.

I then carefully removed the electrical vibrator, and measured approximately the size of the sparking balls. The radiator, it must be remembered, was placed vertically inside a square tube, each of whose sides is 2.5 cm. The radiator was about 1 cm. inside from the free end of the tube.

The diameter of the central ball = 0.78 cm.

„ each side bead = 0.3 „

Distance between the outer surfaces of the beads = 1.5 cm.

„ „ inner (sparking) surfaces „ = 0.9 „

The wave-length, 1.84, is almost exactly equal to twice the distance between the sparking surfaces of the beads. Without further experiments with different sized radiators, it is difficult to say whether the above simple relation is accidental or not. The following rough determinations, made with a second radiator, may be of some interest in connexion with the above. I took off the central sphere from the radiator used in the last experiment, and substituted a larger ball. The distance between the inner sparking surfaces is then 1.2 cm.

Breadth of Strip = 3 cm.

$i.$	$\theta.$	$\lambda.$	Mean.
23.0°	0°	2.34	2.36
29.0	-5	2.38	
34.5	-10	2.36	

The wave-length found is approximately equal to 2.36 cm., and twice the distance between the sparking surfaces is 2.40 cm.

Conclusion.—The experiments described above seem to prove that the diffracted spectrum is not continuous, but linear. The method of determining the wave-length of electric radiation by diffraction grating is seen to give results which are concordant. The determinations are not affected by the periodicity of the receiving circuit, the receiver being simply used as a radioscope. With a better mounting and a finely graduated circle, it would be possible to obtain results with a far greater degree of accuracy. I hope to send, in a future communication, the results obtained with a better form of apparatus, with which I intend to study the relation of the wave-length with the size of the radiator, and the influence of the enclosing tube on the wave-length. I shall at the same time send an account of transmission gratings.

“The Effects of a strong Magnetic Field upon Electric Discharges in Vacuo.” By A. A. C. SWINTON. Communicated by LORD KELVIN, F.R.S. Received June 10,—Read June 18, 1896.

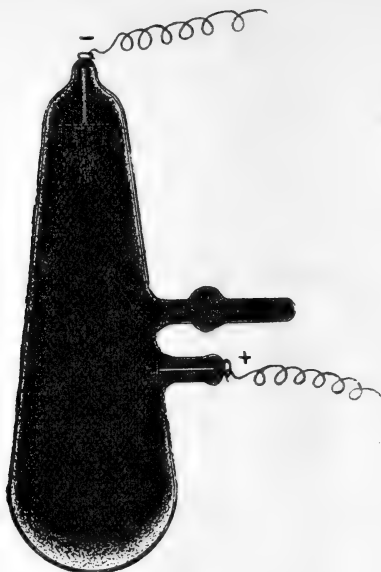
As is well known, when the lines of force of a magnetic field cut the path of the cathode rays in a vacuum tube, the rays are deflected in one direction or another, according to the polarity of the lines of force. If, on the other hand, the relative positions of the vacuum tube and the magnet are such that the lines of force and the cathode rays are parallel, the rays are not sensibly deflected.

Under certain circumstances, however, I have found that with the rays and lines of force parallel, other phenomena occur both in regard to the appearance of the discharge and in connexion with the internal resistance of the tube.

The apparatus employed consisted of a Crookes tube of the form illustrated, supported vertically over one pole of a straight electromagnet. The tube, which was excited by means of a 10-inch Ruhmkorff coil, working much below full power, was about 11 inches in length. The cathode terminal consisted of an aluminium plate at one end of the tube, and the anode a similar plate at one side. The tube was exhausted to a degree that gave considerable green fluorescence of the glass, with a very slight trace of blue luminescence of the residual gas in the neighbourhood of the cathode and anode. The magnet employed had a soft iron core 12 inches in length and $1\frac{1}{2}$ inches diameter. It was wound with 2376 turns of No. 18 S.W.G. copper wire, which, when supplied with continuous electric current at 100 volts pressure, allowed from 13 to 14 ampères to pass, and magnetised the iron core practically to saturation.

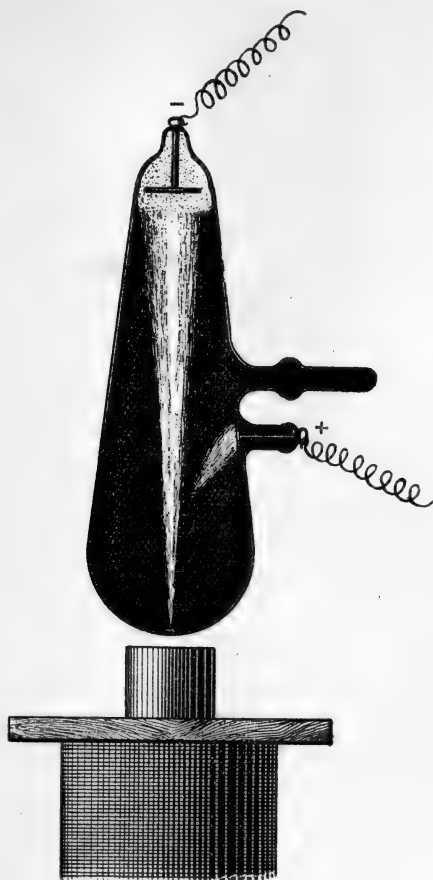
When the Ruhmkorff discharge passed through the tube, the magnet not being excited, the general appearance was as shown in fig. 1, the walls of the tube showing everywhere green fluorescence, which was especially strong all over the rounded end of the tube opposite the cathode. A very small amount of blue luminescence could also be faintly seen just below the cathode, and also in the vicinity of the anode.

With the tube and magnet placed as in fig. 2, as soon as the magnet was excited, the whole appearance of the discharge in the tube was found to alter immediately to what is shown in the illustration. Excepting for a very little at the top of the tube near the cathode, and a very bright spot at the bottom immediately over the magnet pole, all the green fluorescence of the glass disappeared, while extending from near the cathode to the bright spot at the



— Fig. 1. —

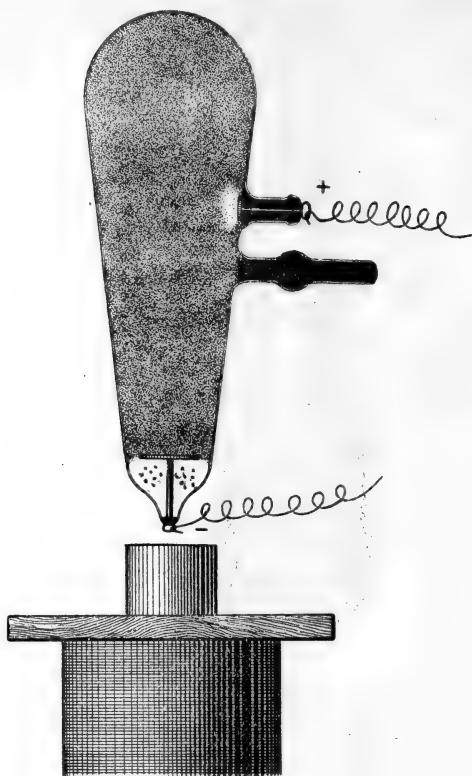
bottom of the tube, a very bright cone of blue luminescence with a still brighter whitish blue core, made its appearance. When under these conditions the tube was slightly moved sideways, the bright spot at the apex of the cone, and the cone itself moved, the spot and apex always maintaining a position exactly over the centre of the magnet pole. At the same time the minor blue luminescence proceeding from the anode terminal, due probably to the "make" current of the Ruhmkorff coil, was bent downwards towards the magnet as shown, and deflected sideways one way or another according to the polarity of the magnet, which polarity, however, did not affect in any way the vertical cathode stream. The internal resistance of the tube, as measured by an alternative spark gap on the Ruhmkorff coil, was also found to be very greatly diminished while the magnet was excited. With the magnet not excited, the alternative spark would leap from $1\frac{1}{4}$ to $1\frac{1}{2}$ inches, while, when the magnet was excited, the gap had to be reduced to about $\frac{1}{8}$ inch before the sparks would pass. As soon as the current from the magnet was cut off, the appearance of the tube immediately reverted to what is shown in fig. 1, and its internal resistance increased to what it had been before.



— FIG. 2. —

Experiments were also tried with the tube reversed as shown in fig. 3. In this case the internal resistance was affected by the magnet just as it had been previously. The appearance of the tube was also altered by the diminution almost to vanishing point of the green fluorescence, the presence of very bright blue luminescence on the under side of the cathode next the magnet, some less bright blue fluorescence near the anode, and a considerable amount of faint blue luminescence throughout the remainder of the tube.

In this case, as in the other, the tube reverted to its normal appearance as soon as the magnet was demagnetised, and the appearance was the same whether the pole of the magnet next the tube was north or south.

— FIG. 3. —

Further experiments with the tube placed horizontally so that the magnetic lines cut the cathode rays produced the usual deflection of the latter, but did not seem to have any appreciable effect on the internal resistance of the tube.

“The Hysteresis of Iron and Steel in a Rotating Magnetic Field.” By FRANCIS G. BAILY, M.A. Communicated by Professor LODGE, F.R.S. Received April 9,—Read June 4, 1896.

(Abstract.)

That the hysteresis of iron varies with the conditions of magnetic change has been ascertained in some instances, notably those in which the attractions between the molecular magnets of the Weber-Maxwell-Ewing theory are diminished by super-imposed vibrations in the

molecules. By deduction from this theory it has been surmised that the hysteresis in magnetic metals under the influence of a constant rotary magnetic field will be less than that in an alternating field in which the magnetising force passes through a zero value. As familiar practical examples of the two conditions may be instanced: the armature core of a continuous current dynamo, and the iron circuit of an alternating current transformer or choking coil.

It is supposed that residual magnetism is due to the combination of molecular magnets in stable magnetic arrangements, and that the energy dissipated in any magnetic change corresponds to the work done in breaking up these arrangements. This energy is rendered kinetic by the movement of the magnets to form new combinations, the magnets either oscillating about the new position or moving to it aperiodically, according to the amount of damping to which they are subject. It is further suggested that the damping is of an electrical or electro-magnetic nature rather than of a frictional character, being produced by the effect of rapid oscillations of the magnets on the surrounding particles or medium. Hence any movement of the molecular magnets during which the formation of new combinations is checked or prevented will take place with considerable reduction in the energy loss due to this cause.

Such a condition is realised when the magnetic substance is subjected to a rotary magnetic field of sufficient strength to force the molecules to maintain a direction parallel to that of the field. If hysteresis is due only to the formation of new combinations and not to mechanical restraint, then under these conditions it will vanish altogether.

Experiments were carried out to verify this deduction. A finely laminated cylinder of iron was suspended on its axis between the poles of an electro-magnet which was capable of rotation about the axis of suspension of the cylinder, thus producing a magnetic field rotating in a plane at right angles to this axis. The cylinder, though otherwise free to rotate, was restrained from continuous rotation by a spring, and the angle of rotation and consequent restoring force of the spring was indicated by a beam of light reflected from a mirror on the cylinder. The speed of the electro-magnet and the exciting current could each be varied.

On rotating the magnet, the armature was dragged round until the restoring force of the spring equalled the force due to hysteresis, and the value of the latter could be obtained from the observed deflexions. The result showed that the value of the hysteresis under these conditions was very different from that obtained in an alternating field. At first the value was higher for corresponding inductions, but at an induction of about 16,000 in soft iron and 15,000 in hard steel the hysteresis reached a sharply defined maximum and rapidly dimin-

ished on more complete magnetisation, until at an induction of about 20,000 it became very small with every indication of disappearing altogether. Soft iron and hard steel gave very similar curves, and in both the curve of hysteresis-induction cut the curve obtained from the values in an alternating field at a point just before the maximum. The result fully bears out the deduction from the theory, and proves in addition that hysteresis is not sensibly due to anything of the nature of mechanical restraint of the molecules. The form of the curve also gives clear indications of the three stages of molecular movement, the first stage giving a slowly rising curve, the second a straight rapid rise, and the third a straight and much more rapid descent.

Further experiments were carried out on the effect of speed of rotation. In an alternating field the speed of reversal has been shown to be without sensible effect on the hysteresis, and theory points to this result as a natural deduction. The above apparatus was well adapted for testing the matter, since the hysteresis per reversal could be read at each instant independently of the speed. From an extremely slow speed up to 70 revolutions per second no definite change was found in the value of the hysteresis. At the same time several small modifications were noted, produced by rapid variations in the speed of rotation or magnetising force. The effect lasted through many revolutions, but ultimately the same steady condition was arrived at. At and near the maximum value the hysteresis was very variable. The effects were much more marked in soft iron than in hard steel, as would be anticipated from the theory of their constitution.

The experiments in their verification of an untried deduction form a strong proof of the validity of the molecular theory of magnetism, and throw some light on the nature of the molecular complex and of the interactions which take place therein.

“A Magnetic Detector of Electrical Waves and some of its Applications.” By E. RUTHERFORD, M.A., 1851 Exhibition Science Scholar, New Zealand University, Trinity College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received June 11,—Read June 18, 1896.

(Abstract.)

The effect of Leyden jar discharges on the magnetisation of steel needles is investigated, and it is shown that the demagnetisation of strongly magnetised steel needles offers a simple and convenient means for detecting and comparing currents of great rapidity of alternation.

The partial demagnetisation of fine steel wires, over which is wound a small solenoid, was found to be a very sensitive means of detecting electrical waves at long distances from the vibrator. Quite a marked effect was found at a distance of over half a mile from the vibrator.

Detectors made of very fine steel wire may be used to investigate waves along wires and free vibrating circuits of short wave-length. Fine wire detectors are of the same order of sensitiveness as the bolometer for showing electrical oscillations in a conductor.

This detector also has the property of distinguishing between the first and second half oscillations of a discharge, and may be used for determining the damping of electrical vibrations and the resistances of the discharge circuit.

A method of experimentally determining the period of oscillation of a Leyden jar circuit by the division of rapidly alternating currents in a multiple circuit is explained. The capacity and the self-inductance of the circuit for high frequency discharges may also be deduced, so that all the constants of a discharge circuit may be experimentally determined. In the course of the paper the following subjects were investigated.

(1) *Magnetisation of Iron by High Frequency Discharges.*—The effect of the Leyden jar discharge on soft iron and steel is fully examined. Steel needles which had been placed in a solenoid and subjected to a discharge were examined by dissolving them in acid. It was found that there was apparently only evidence of two half oscillations in the discharge, and this effect is due to the demagnetising force exerted by the needle on itself during the discharge.

The effect of continued discharges on the demagnetisation of magnetised steel needles was investigated, and also the effect of varying the length and diameter of the steel needles.

When a discharge is sent *longitudinally* through a magnetised steel wire the magnetic moment of the needle is always decreased, due to the circular magnetisation of the wire by the current through it. This "longitudinal" detector, when of thin steel wire, was found to be a sensitive means of detecting electrical oscillations of small amplitude.

Both the "longitudinal" and "solenoidal" detectors may be readily used for comparing the intensities of currents in multiple circuits when traversed by currents of the same period.

(2) *Detection of Electrical Waves at Long Distances from the Vibrator.*—A compound detector needle was composed of fine steel wires and a small solenoid wound over it. When this detector was placed in series with the wires of a receiver, the electrical oscillations set up in the circuit tended to demagnetise the magnetised detector needle.

By this method electrical waves from a Hertzian vibrator were

detected for long distances. An effect was obtained at over half a mile from the vibrator.

(3) *Waves along Wires*.—The uses of fine steel wires for examining the distribution of currents along wires are explained.

(4) *Damping of Oscillations*.—A method of determining the damping of discharge circuits is investigated. The absorption of energy in spark gaps is deduced, and the apparent resistance of the air break to the discharge determined.

(5) *Resistances of Iron Wires*.—Quantitative results are given for the resistance of iron wires for very rapid alternations. The value of the permeability of the different specimens is deduced, and it is shown to vary with the diameter of the wire and the intensity of the discharge.

(6) *Absorption of Energy by Conductors*.—The absorption of energy of iron and non-magnetic cylinders placed in solenoid through which a discharge passed were determined. Iron cylinders were found to absorb much more energy than copper ones of the same diameter, and the permeability of the iron for the discharge is deduced.

(7) *Determination of the Period of Oscillation of Leyden Jar Discharges*.—A method of accurately determining the period of oscillation is based on the division of rapid alternations in a multiple circuit, one arm of which is composed of a standard inductance, and the other of a variable electrolytic resistance.

The value of n , the number of oscillations per second, when the currents in the branches of the multiple circuits are equal, is, under certain conditions, given by—

$$n = \frac{R}{2\pi W},$$

where R = resistance of electrolyte to the discharge,

W = value of the standard inductance.

The value of the self-inductance and capacity of the discharge circuit for very rapid oscillations may also be experimentally deduced.

“Magnetisation of Liquids.” By JOHN S. TOWNSEND, M.A.
Dub. Communicated by Professor J. J. Thomson, F.R.S.
Received June 11,—Read June 18, 1896.

(Abstract.)

The experiments on the coefficient of magnetisation of liquids were made with a sensitive induction balance. Both circuits were commuted about sixteen times a second, so that very small inductances could be detected by the galvanometer in the secondary circuit. The principle of the method consisted in balancing the increase of the

mutual induction of the primary on the secondary of a solenoid arising from the presence of a liquid in the solenoid against known small inductances. Thus, if the sum of the inductances be reduced to zero, as shown by the galvanometer in the secondary giving no deflection, the balance will be disturbed to the extent $4\pi kM$, due to the insertion of a liquid into the solenoid whose coefficient of magnetisation is k , and the galvanometer in the secondary circuit will give a deflection when the commutator revolves. An adjustable inductance is then reduced by a known amount, m , till the deflection disappears; so that we get

$$4\pi kM = m \quad \therefore k = m/4\pi M,$$

where m and M are quantities easily calculated.

Since the formula does not contain either the rate of the rotation of the commutator nor the value of the primary current, no particular precautions are necessary to keep these quantities constant.

In all the determinations the magnetising force was varied from 1 to 9 centigram units, and in no case was there any variation in k . The densities of the salts in solution were also varied over large ranges, and showed that the coefficient of magnetisation for ferric salts in solution depended only on the quantity of iron per c.c. that was present, giving the formula

$$10^7 k = 2660 W - 7.7$$

for ferric salts, where W is the weight of iron per c.c., the quantity -7.7 arising from the diamagnetism of the water of solution.

A similar result was obtained for ferrous salts, the corresponding formula being

$$10^7 k = 2060 W - 7.7,$$

the temperature being 10°C .

The following table shows the coefficient of magnetisation for the different salts examined, w being the weight of the salt per c.c. of the solution:—

	$10^7 k.$
Fe_2Cl_6	$916 w - 7.7$
$\text{Fe}_2(\text{SO}_4)_3$	$745 w - 7.7$
$\text{Fe}_2(\text{NO}_3)_6$	$615 w - 7.7$
FeCl_2	$908 w - 7.7$
FeSO_4	$749 w - 7.7$

The effect of temperature was also estimated, the results of the experiments being shown by means of a curve (fig. 1), the x ordinates of which denote the temperature, and the y ordinates are proportional to the coefficient of magnetisation, a length corresponding to 50 being subtracted from each for convenience of representation.

The first is drawn from results of experiments performed on ferric chloride containing 0.086 gram of iron per c.c., the second from ferrous chloride containing 0.148 gram of iron per c.c., the third from ferric sulphate containing 0.105 gram of iron per c.c., and the fourth from an alcoholic solution of ferric chloride.

The curves all show about the same temperature coefficient at points corresponding to the same temperature.

“On Fertilisation, and the Segmentation of the Spore, in *Fucus*.” By J. BRETLAND FARMER, M.A., Professor of Botany at the Royal College of Science, and J. LL. WILLIAMS, Marshall Scholar at the Royal College of Science, London. Communicated by D. H. SCOTT, M.A., Ph.D., F.R.S. Received May 21,—Read June 18, 1896.

The object of the present communication is to give an account of the chief results of an investigation into the processes connected with the formation and fertilisation of the oospheres and the germination of the spore in *Ascophyllum nodosum*, *Fucus vesiculosus*, and *Fucus platycarpus*. The more obvious details of development have been especially studied by Thuret, and later by Oltmanns. But neither of these writers paid any special attention to the behaviour of the cell-nuclei, nor did they succeed in observing the actual process of fertilisation. Behrens has communicated an account (‘Ber. d. Deutschen Bot. Gesel.,’ Bd. IV) of some researches made by himself on the fertilisation of the oospheres, but we are unable to accept his conclusions for reasons shortly to be recounted.

The material for these investigations was obtained in London from Bangor, Plymouth, and Jersey, but it was compared with other material collected and fixed at the seaside at Bangor, Weymouth, and Criccieth. Furthermore, all the growing apices and conceptacles for sectioning were collected by one of us directly at the three last named places. Some samples were gathered between the tides, and fixed at once, others were first kept for a time in salt water; the best results, however, were obtained from plants collected in a boat about two or three hours after the tide had reached the plant, and also from other plants taken a short time before they were left exposed by the ebb tide.

In order to study the fertilisation and germination stages, male and female plants were kept in separate dishes, and were covered over so as to prevent drying up. This method gave far better results than those more usually advocated. On the appearance of the

extruded sexual products, the female receptacles were placed in sea-water, and after the complete liberation of the oospheres, a few male branches with ripe antherozoids were first placed in a capsule of sea water until it became turbid owing to their number. If on examination the antherozoids proved to be active, small quantities were added to the vessels containing the oospheres. The latter were then fixed at intervals of five minutes during the first hour, and then at intervals of fifteen minutes, up to six hours after the addition of the antherozoids. After that, samples were killed at longer intervals up to three days, and this was continued till we had material fixed at all stages for the first fortnight. At first we used sea water in which to keep the embryos growing, but a proper solution of Tidman's sea salt was found to answer quite as well.

For fixing, we tried the following reagents—chrome alum, picric alum, Mann's picro-corrosive, corrosive sublimate, and acetic acid; these were all dissolved in sea water, absolute alcohol, Flemming's and Hermann's solutions, and the vapour of osmic and formic acids. The Flemming's (strong formula) and Hermann's solutions were diluted with equal parts of sea water. The first three fixatives were unsuccessful, acetic-corrosive yielded fair nuclear figures, but the material proved very brittle, and the spores were somewhat distorted. A portion of the cytoplasm was disorganised and the polar radiations were not preserved. Absolute alcohol fixed the oospheres and newly fertilised spores without distortion, but was useless for all other stages. Vapour fixing with osmic acid succeeded better than any of the preceding reagents but was greatly inferior to either Hermann's or Flemming's solutions in preserving the protoplasmic structure in an unaltered state.

After the material had been fixed it was dehydrated and passed in the usual way into paraffin, the temperature of which was not allowed to exceed 50° C., and it was then cut with the microtome. The sections were stained with Heidenhain's iron-hæmatoxylin, with Flemming's triple stain, and a large number of other dyes. The results, which were compared carefully, led us to rely chiefly on the two staining processes mentioned, but at the same time we often obtained valuable preparations with other staining reagents as well.

In spite of repeated attempts, we have not succeeded in observing the first nuclear division in the oogonium, but the later ones have been seen both in *Fucus vesiculosus* and in *F. platycarpus*, in which eight oospheres are formed. Oltmanns asserts that in *Ascophyllum*, in which only four oospheres are commonly formed, eight free nuclei occur at an earlier stage, but that four of these ultimately abort, and do not become centres of cell formation. Our observations tend to confirm him in this respect, but we found that in some cases a fifth oosphere, smaller than the rest, was occasionally differentiated,

and that when freed from the oogonium it exerted an attraction on the antherozoids just like its larger sister oospheres.

When an oogonial nucleus is about to divide, it first becomes slightly, then very much, elongated so as to resemble an ellipse. Fine radiations are seen to extend from the two ends into the surrounding cytoplasm. The latter is at first tolerably uniformly granular, but as the radiations around the polar areas increase, these regions become cleared altogether of the granules which then become massed outside them. The nucleus rapidly becomes more spindle-shaped, and its chromatic elements are chiefly grouped near each pole, leaving a clear space about the equator in which the nucleolus is situated. In this respect the nuclei of *Fucus* offer a striking contrast to those of *Pellia epiphylla* already described ('Annals of Botany,' vol. viii, p. 221) by one of us. In the latter plant the chromatic portion of the nucleus assumes an equatorial position at the corresponding stage in division, whilst the polar regions are clear.

The polar radiations continue to increase and the nucleus to lengthen, until the entire structure recalls the figure of a dumb-bell, in which the nucleus answers to the handle, and the radiation areas to the knobs. If the radii be traced outwardly, they are seen to terminate either in the frothy protoplasm, on the angles where the foam walls meet, or on the large granules which surround the cleared areas and are embedded in the foam. This point is one of considerable importance, and we shall revert to it further on. No structures were seen which could *certainly* be identified as centrosomes, although bodies suggestive of them were often observed; but these proved to be so variable in size and position, as well as in number, that we feel unable to attach any special significance to them.

The next stage in the mitosis is that in which the interpolar spindle arises, with the chromosomes disposed upon its equator. The spindle is very remarkable inasmuch as it is entirely intranuclear, somewhat resembling that described by Fairchild for *Valonia*, or by Harper for *Peziza*. The nuclear wall can be distinguished until quite late in karyokinesis, and it is possible that no complete mingling of the cytoplasm with the contents of the nucleus takes place here. The spindle is extremely clear, and in several preparations, owing to a fortunate contraction during manipulation, the ends of the nuclear part of the spindle also had broken away from the cytoplasmic poles, and were visible as clean conical structures forming the poles of the nuclear spindle. The chromosomes were too minute to admit of their development being satisfactorily studied, but in *all the oogonial spindles* their number was estimated at *ten* when seen arrayed on the spindle equator. They were only seen in profile, and consequently it was difficult to be sure whether there

were really ten or twelve, but the absolute number is not of importance as all the nuclei were compared from the same aspect. Remains, more or less preserving the original form, of the nucleolus were sometimes visible at this and even in a later stage. No division-planes are formed in the oogonium until the full complement of nuclei are produced; after this the positions which they will ultimately occupy are indicated by the heaping up into lines (or rather plates) of the cytoplasmic granules above referred to. These seem to be repelled equally from all the nuclei, thus effecting a symmetrical division of the entire oogonium.

After the complete delimitation of the oospheres within the oogonium, we observed, as an occasional circumstance, that one of the oospheres might contain two, or even three, nuclei, a fact also noticed by Oltmanns. When the oospheres are extruded, and come to lie free in the water, they grow in size, and are turbid with granules, which are very abundant in the cytoplasm. The chromatophores early become distinguishable from the other constituents of the cell, and the nucleus occupies a central position. It is itself surrounded by a dense layer of cytoplasm, which later on becomes very strongly marked. About five minutes after the mixing of the sexual cells, the antherozoids are found to have slipped into many of the oospheres. We failed to observe the act of penetration, but found a number of cases in which the antherozoid could be recognised within the oosphere, before its final fusion with the nucleus of the latter. It is a roundish, densely staining body, and, unlike the majority of animal sperm cells as yet described, it imports into the egg no system of radiations along with it. Judging from the short period of time elapsing between its penetration of the surface of the oosphere and its arrival at the exterior of the female nucleus, it must pass through the intervening cytoplasm with great rapidity. It then becomes closely appressed to the nucleus, and is about as large as the nucleolus of the latter. It rapidly spreads over a part of the female nucleus as a cap, and it presents a less homogeneous aspect than before. Both it and the female nucleus assume a granular condition, which is probably to be interpreted as representing a coiling and looping of the lining of the respective nuclei. Finally the two nuclei coalesce, and the original components can no longer be distinguished. Complete fusion may be effected in less than ten minutes after addition of the antherozoids to the water. These results are in striking accordance with those described by Wilson in connexion with the fertilisation of the eggs of echinoderms in his recent "*Atlas of Fertilisation.*"

A delicate pellicle is meanwhile formed around the periphery of the oosphere, which is thus easily distinguished from the unfertilised oospheres, in which such a membrane is wanting. The texture of the

cytoplasm also changes, and tends to assume a more definitely radiating character, the lines starting from the nucleus as a centre.

We observed, not unfrequently, rather large cells in which two nuclei of equal size were lying in close juxtaposition. These cells, with their nuclei, answer exactly to the description given by Behrens of the fertilisation stage in plants examined by him. We are unable however, to accept his interpretation, for, in the first place, the series of fertilisation stages which we have observed, and have briefly described above, in no way correspond with the appearances described by him, and secondly, because these large cells (Behrens himself emphasises their size) are seen in material to which no antherozoids have had access. Furthermore, the average size of the young oospores is *not* obviously greater than that of the oospheres themselves. We regard the bodies in question as representing abnormal developments of oogonial cells, and not as being in any way concerned with fertilisation. Moreover, we have occasionally observed one cell in the divided oogonium, much larger than the rest, to contain two, or even sometimes three, nuclei, and these nuclei are then always close together. These facts have led us to reject Behrens' account of the process.

A very large number of experiments were made, in order to determine, if possible, the time which elapsed between the addition of the antherozoids to the oospheres and the first division of the spore. A short summary of different sets of observations on *Ascomyllum* is given in the subjoined tables.

SERIES I.—*Observations on Ascomyllum conducted at the Seaside.*

(a) The antherozoids were added to the oospheres at 10 o'clock A.M.

Lot 1.	Fixed 23 hours after the addition of antherozoids.	Nucleus preparing for division.
" 2.	" 24 " " "	Nucleus divided, rhizoid rudiment present, no dividing wall.
" 3 & 4	" 32 " " "	Nucleus divided, no rhizoid, dividing wall present.
" 5.	" 36 " " "	Spore divided into about six cells.

(b) The antherozoids added between 11 and 12 P.M.

Lot 1.	Fixed 24 hours after the addition of antherozoids.	Nucleus divided, a few with rhizoid rudiments and division wall.
" 2.	" 25 " " "	Same result.
" 3.	" 25 " " "	Not beyond spindle stage.
" 4.	" 28 " " "	Nucleus divided, no rhizoid or dividing wall.

SERIES II.—*Observations on Ascophyllum carried on in the Laboratory.*

Antherozoids added between 5 and 7 P.M.

Lot 1.	Fixed	22½	hours	after	the	addition	of	antherozoids.	Nucleus divided, no rhizoid or dividing wall.
„ 2.	„	23	„	„	„	„	„	„	Nucleus preparing for division.
„ 3.	„	23	„	„	„	„	„	„	Same as 1.
„ 4.	„	24½	„	„	„	„	„	„	Nucleus divided, rhizoid present, no dividing wall.

The above observations prove that there is no essential difference between the behaviour of material examined in London and at the seaside respectively.

After fertilisation, the cells rest for a long interval of time—commonly about twenty-four hours, as shown in the foregoing table—before they begin to segment. The principal changes which occur during the interval are, first, in the rapid increase in the thickness of the peripheral cell wall, and, secondly, in the more regular arrangement of structure exhibited by the protoplasm. The alveolar, or foam character is extremely clear, and the chromatophores, which by this time have become very prominent, are noticed to be situated in the angles formed by the convergence of the foam walls; they are often bent and otherwise distorted, and so accommodate themselves to the structural condition of the foam. Other granules, which stain deeply, and probably represent food reserve of a proteid nature, are also abundantly scattered through the cytoplasm.

The first segmentation-division resembles, in a general way, the oogonial nuclear divisions already described, and the polar areas become similarly cleared of granules. The achromatic threads forming the polar radiations are very clearly seen to be attached to the foam-like structure of the cytoplasm, and, indeed, in some cases, insensibly to pass into it. At other times fibrils end on granules (or, perhaps, on the protoplasmic lining of the granules), and sometimes again a fibril may fork, and its branches end either on granules or on the foam angles. The inference to be drawn from these facts seems to be that the radiations are the result of a change—a differentiation—in the protoplasm as it already exists, and that they do not owe their origin to the presence of any special “spindle-forming substance,” by virtue of which they may be supposed to develop and “grow” as new structures in the cell. We propose, however, to discuss the general bearings of our observations on this and on other questions of theoretical interest in a future memoir, in which the evidence for our views will be set forth in detail.

When the achromatic nuclear spindle appears, it also, as in the

oogonial mitoses, is intranuclear, and it is often separated from the well-defined persistent nuclear wall by a clear space. The chromosomes, when assembled on the spindle, at the equator, are seen to be *twice as numerous* as in the oogonial nuclei, *i.e.*, seen in profile we counted them as *twenty* in number. We were unable to distinguish any such grouping of the chromosomes as would lead to the conclusion that the chromosomes of the male and female nuclei respectively had so far preserved their original identity as to appear in the form of two separate groups. The long interval of time which, in *Fucus*, elapses between fertilisation and the first nuclear division possibly may admit of a more thorough mingling or fusion of the parental chromosomes than would seem to be the case in some animals, *e.g.*, the Copepoda as described by Rückert and by Häcker.

During the diaster stage the connecting achromatic fibres are at first very distinct, but they soon become fainter, and no cell-plate is formed across them. The two daughter nuclei gradually pass into the state of rest, each being first hemispherical, with crenate projections on the flattened side turned towards its sister nucleus. Only after nuclear division is complete does the first cell wall appear. The cell is sometimes spherical when this happens, and then it is divided into two similar hemispheres. Further divisions may then appear, whilst the general contour of the embryo still remains more or less spherical. These cases occurred most frequently when the germinating spores were illuminated on all sides. But most commonly the first cell wall cuts the spore into two dissimilar halves, one of which grows out and forms a rhizoid. Often this projection is already apparent even before the first nuclear division occurs, and in any case one of the two daughter nuclei always passes down into the protuberance.

The immediately succeeding divisions have been sufficiently described by Thuret and others, but we may remark that the division of the nuclei in all cases precedes the formation of a cell plate, which is not formed in connexion with the achromatic connecting fibrils as in the higher plants.

The doubled number of the chromosomes is retained during the vegetative divisions of the thallus, and is constant throughout the somatic cells of the mature *Fucus* plant. Hence it follows that the reduction in the number of the chromosomes (in the female plants), is associated with the differentiation of the oogonium—the mother cell of the sexual products. Thus *Fucus*, in this respect, approximates more closely to the type of animal oogenesis than to that which obtains in those higher plants in which the details of chromosome reduction has been followed out.

Regarded from the standpoint of the number of its chromosomes, the *Fucus* plant resembles the *sporophyte* of the higher plants, whilst

the gametophyte of the latter, with its reduced number of chromosomes, finds its analogue merely in the maturing sexual cells of *Fucus*. But until we know more of the nuclear changes as they occur in other *Algæ*, and especially in the more primitive forms, it seems unadvisable to go further than to indicate the possibility that we may require to revise our present ideas on the comparative morphology of the higher and lower groups of the vegetable kingdom. Even if we regard the reduction in the number of the chromosomes as a fact which is primarily of physiological importance, we may safely conclude, from the universality of its occurrence, that it is also intimately connected with the phylonogenetic development of living forms, and hence it must meet with due recognition on the part of the morphologist who is engaged in comparing the life-history of one group of organisms with that of others.

“On certain Changes observed in the Dimensions of Parts of the Carapace of *Carcinus maenas*.” By HERBERT THOMPSON. Communicated by Professor W. F. R. WELDON; F.R.S. Received May 19,—Read June 11, 1896.

In making some measurements of young male *Carcinus maenas* from Plymouth, corresponding to those made by Professor Weldon on young females of the same species, and published by him in the Report of a Committee for conducting statistical inquiries into the measurable characteristics of plants and animals (*‘Roy. Soc. Proc.’*, vol. 57, p. 360), some interesting facts were observed as to changes taking place in the relative dimensions of certain parts of the carapace of these crabs in the space of the last three years.

The carapace of the adult male crab, measured in the median antero-posterior line is, roughly, from 40 to 60 mm. long. Now, of young male *C. maenas* collected at random at Plymouth in the year 1893, I had, for the purposes of measurement, 3,077 specimens, ranging between 10 and 15 mm. in length of carapace, and on these, besides the carapace length, as above defined, two other measurements were taken, viz. (1) “frontal breadth,” the distance in a straight line between the tips of the two teeth which form the outer boundaries of the orbits, and (2) the “right dentary margin,” measured in a straight line from the tip of the first to that of the last lateral tooth on the right side of the carapace.

The measurements were made in the way described in the Report above mentioned (*ibid.*, pp. 361—2): and owing to the rapid growth and alteration of proportional dimensions in the young crabs, they were sorted into groups, the members of each of which differed by less than 0.2 mm. in carapace length, thus giving five groups for

every 1 mm. of growth in carapace length, or twenty-five groups for the whole range of 10—15 mm. carapace length. The numbers contained in the separate groups ranged from seventy-two in the smallest group to 178 in the largest group. The arithmetical mean and mean error in each group is set out in Table I *infra*.

Similar measurements were made in the case of 1,957 young male *C. mænas* from Plymouth of the year 1895. These were likewise divided into groups differing by 0·2 mm. of carapace length: and the numbers contained in the twenty-five groups between 10 and 15 mm. carapace length ranged from thirty-four in the smallest one to 111 in the largest. The arithmetical means and mean errors are given in Table I *infra*.

On comparing the two sets of measurements (expressed in terms of the carapace length which was taken as the unit) it appears, as regards the "frontal breadth," that in every one of the twenty-five groups without exception the average size of the frontal breadth in the 1893 crabs exceeded that of the 1895 crabs of corresponding size. Seeing how small the groups are the result is a striking one, and is given in greater detail in the following Table:—

C. mænas.—Frontal Breadth.

Carapace length in millimetres.	Average excess of 1893 crabs over 1895 crabs.	
	In thousandths of carapace length.	In millimetres.
10—11	6·30	0·07
11—12	7·29	0·08
12—13	6·73	0·08
13—14	5·26	0·07
14—15	3·53	0·05

On the other hand, if the species in 1895 has a smaller average frontal breadth, it compensates for the deficiency by having a larger right dentary margin. This was found to be the case in twenty-three out of the twenty-five groups, the two non-conformist groups lying one near each end of the range. The arithmetical means and mean errors are given in Table I *infra*, and the results, tabulated in a corresponding form to those of the frontal breadth measurements, are as follows:—

C. maenas.—Right Dentary Margin.

Carapace length in millimetres.	Average excess of 1895 crabs over 1893 crabs.	
	In thousandths of carapace length.	In millimetres.
10—11	1·39	0·01
11—12	2·09	0·02
12—13	1·87	0·02
13—14	1·56	0·02
14—15	1·42	0·02

As these results seemed to indicate that a change in regard to these dimensions was taking place in the species, it was desirable to compare similar measurements in the adult. Fortunately Professor Weldon was able to supply me with 254 specimens of male *C. maenas* with a carapace length ranging between 40 and 63 mm., taken at Plymouth at random in 1892-3: and for comparison he procured 496 individuals collected at Plymouth in January of the present year and corresponding in size.

Measurements similar to those made on the young ones gave the following results:—In frontal breadth the 1892-3 crabs exceeded the 1896 crabs on an average by 8·85 thousandths of their carapace length, which for an average length of 50 mm. is equivalent to 0·44 mm., while in the right dentary margin the 1896 crabs exceeded those of 1892-3 on an average by 3·1 thousandths, or an equivalent of 0·16 mm., thus fully confirming the results arrived at in the young ones.

Whether these results indicate a permanent change in the species at Plymouth in respect to these particular dimensions of the carapace, tending to the establishment of a new variety, or whether it is a mere oscillation such as, for all we know, may be constantly going on in the relative dimensions of the various parts of the members of all species, can only be decided by further measurements, which, it is hoped, may be continued on the same species after another interval of two or three years. Meanwhile, the persistence with which the same tendency asserts itself in the twenty-six groups into which we have divided these crabs of 1892-3 and 1895-6 is remarkable, and may perhaps induce others to take measurements of other animals at definite intervals, and establish similar comparisons.

I wish to add my hearty thanks to Professor Weldon for suggesting the line of investigation and furnishing material and ever-ready help.

Table I.

Mean length.	1893—(3077 ♂).				1895—(1957 ♂).			
	Number of animals.	Frontal breadth.		Right dentary margin.	Number of animals.	Frontal breadth.		Right dentary margin.
		Arithmetic mean.	Mean error.			Arithmetic mean.	Mean error.	
10.1	72	816.17	9.75	413.25	53	809.08	9.77	414.45
10.3	87	812.06	11.90	414.87	62	804.82	9.99	416.36
10.5	107	807.37	10.79	418.87	79	803.27	9.83	417.70
10.7	143	808.96	10.14	417.11	64	801.69	8.64	421.06
10.9	145	805.07	10.13	419.00	66	799.27	9.70	420.49
11.1	169	802.50	11.34	420.28	92	794.12	8.10	424.17
11.3	178	798.18	10.11	422.45	79	792.38	7.92	423.27
11.5	161	797.19	10.26	421.94	85	788.83	8.08	424.06
11.7	167	794.28	11.10	423.92	79	785.29	7.39	425.61
11.9	146	791.45	10.82	426.18	107	786.53	8.13	428.12
12.1	149	788.38	10.58	427.10	101	780.61	8.92	429.67
12.3	159	783.98	10.56	427.57	94	779.50	9.20	428.44
12.5	158	783.99	10.80	428.87	111	776.50	7.85	430.02
12.7	117	783.58	11.45	430.29	105	773.43	8.44	432.91
12.9	130	777.38	9.95	432.23	98	773.63	8.43	434.40
13.1	109	776.63	9.84	432.05	90	771.61	8.04	432.50
13.3	96	774.60	11.20	434.71	76	766.21	8.22	436.47
13.5	114	766.91	12.21	434.85	79	763.96	7.85	435.86
13.7	119	767.63	11.82	436.24	98	762.00	8.57	438.68
13.9	98	763.73	12.20	438.48	77	759.40	8.15	440.64
14.1	98	758.94	12.65	440.52	67	757.00	8.06	440.58
14.3	99	756.90	11.73	440.74	69	755.77	9.19	441.64
14.5	83	762.60	11.86	440.86	55	754.45	7.40	443.73
14.7	85	763.00	11.36	443.41	37	749.84	8.59	447.41
14.9	88	751.32	10.91	444.73	34	748.03	7.27	444.00
Adult	254	600.45	15.00	496.55	496	591.60	12.75	499.65

Note.—The arithmetic means are expressed in terms of the carapace length, which is here taken as = 1000.

“Phenomena resulting from Interruption of Afferent and Efferent Tracts of the Cerebellum.” By J. S. RISIEN RUSSELL, M.D., M.R.C.P., Research Scholar to the British Medical Association, Assistant Physician to the Metropolitan Hospital, and Pathologist to the National Hospital for the Paralysed and Epileptic, Queen’s Square. Communicated by Professor VICTOR HORSLEY, F.R.S. Received June 17,—Read June 18, 1896.

(From the Pathological Laboratory of University College, London.)

(Abstract.)

The research was undertaken in the hope of obtaining evidence in support of or against the view that the cerebellum exercises a direct influence on the spinal centres, as opposed to any indirect influence exerted through the agency of the cerebral cortex. The inferior peduncle of the cerebellum was accordingly divided on one side, the organ itself and its other peduncles being otherwise left intact, and the results obtained by this procedure were controlled by experiments in which the lateral tracts of the medulla oblongata were divided on one side without injury to the pyramid on the one hand or to the posterior columns and their nuclei on the other. Further control experiments consisted in dividing transversely the posterior columns and their nuclei a few millimetres above the calamus scriptorius, on one side, without including the lateral tracts of the medulla in the lesion.

The results obtained by these different experiments were supplemented by others in which the electrical excitability of the two cerebral hemispheres was tested and compared, immediately after division of one inferior peduncle of the cerebellum, and at some later period, such as three weeks, after the section of the peduncle; also after partial hemisection of the medulla in which all the structures on one side were divided, with the exception of the pyramid which was left as far as possible intact.

Other experiments consisted in observing the ways in which convulsions, induced by the intravenous injection of the essential oil of absinthe, were modified by division of one inferior peduncle of the cerebellum, by partial hemisection of the medulla in which the pyramid was the only structure left intact on one side, and by transverse section of the posterior columns and their nuclei, on one side, a few millimetres above the calamus scriptorius.

Considered in conjunction with results previously obtained by the author and others after ablation of one lateral half of the cerebellum, and after intracranial section of the auditory nerve, the results now

obtained afford valuable information with regard to many of the functions of the cerebellum; but they are not claimed as supplying definite information on the important question as to whether the cerebellum exercises a direct downward influence on the spinal centres or not. Many of the results obtained suggest the possibility of such a downward influence; but most of the effects can as readily be explained by supposing that they are the result of the interruption of afferent impulses passing from the periphery to the cerebellum.

The direction of rotation was towards the side of the lesion after division of one inferior peduncle, or in other words if, as was always the case, the left peduncle was divided, the animal rotated like a right handed screw entering an object. The direction of rotation was thus the same as after intracranial section of the auditory nerve, and the reverse of what results on ablation of one lateral half of the cerebellum. The bulk of the afferent impulses, whose interruption is responsible for this phenomenon, probably reach the inferior peduncle from the auditory nerve, but that all the impulses are not derived from this source was shown by the fact that lateral section of the medulla below the auditory nerve and its nuclei may result in similar rotation.

The disorders of motility which followed division of one inferior peduncle corresponded exactly with those observed after ablation of one lateral half of the cerebellum. In view of the results obtained by Claude Bernard, and by Mott and Sherrington, as regards impairment of movement after section of sensory spinal roots, it is suggested that the defects of movement which result from section of one inferior cerebellar peduncle may be due to the interruption of such afferent impulses passing to the cerebellum, rather than to the cutting off of efferent impulses from the cerebellum to the spinal centres. The way in which the sensory defects correspond in distribution to the motor, and the fact that recovery of sensory conduction commences before any improvement in motor power can be detected, are held to support this view.

Cutting off of some afferent impulses can alone be considered responsible for the ocular displacements met with. These displacements correspond with those which are the result of ablation of one lateral half of the cerebellum, the displacement of the globes being downward and to the opposite side from the lesion. The displacements following lateral section of the medulla were the same; but after division of the posterior columns and their nuclei on one side, the displacement of the globes was downward and to the side of the lesion.

Spasm, which was easily detected in the back and neck muscles on the side of the lesion, causing incurvation of the vertebral axis to that side, alone furnished any satisfactory information in support of the

possible control which the cerebellum may exert on the spinal centres. The state of the knee-jerks afforded no satisfactory information on this point.

The blunting of sensibility met with is held to be further proof that the cerebellum is concerned with sensory as well as motor processes, as was contended by the author in a former paper.

Faradic excitability of the opposite cerebral hemisphere was found to be less than of that on the side of the lesion, both when the inferior cerebellar peduncle was divided, and when partial hemisection of the medulla was performed, leaving the pyramid intact. The most satisfactory explanation of this phenomenon appears to be that the removal of some afferent inhibitory influence from one half of the cerebellum allows this half of the organ to further inhibit the cortex of the opposite cerebral hemisphere; an explanation in keeping with that offered when the results of ablation of the cerebellum were under consideration.

This view is strengthened by the remarkable results obtained by the intravenous injection of absinthe in animals in whom the same lesions had been previously produced, for with the pyramidal system absolutely intact on both sides, there was an entire absence of contraction of the muscles of the anterior extremity on the side of the lesion, and diminution of contraction of the muscles of the posterior extremity on this side, as compared with those of the opposite limb. Such was the result obtained when the convulsions were induced soon after the lesion, but when induced at some remote period, such as three weeks after, the muscles of the anterior extremity on the side of the lesion contracted, though the contractions were much less powerful than were those of the opposite anterior extremity, and were often largely tonic in character.

Transverse section of the posterior columns, and their nuclei alone on one side, did not alter the character of the absinthe convulsions in such a remarkable manner as did division of the peduncle and lateral section of the medulla. After such a lesion the muscular contractions in the anterior extremity on the side of the lesion were less powerful than were those in the opposite anterior extremity, and there was more tonus and less clonus than in the contractions on the opposite side. Both these characters were evident in the early convulsions of a series, but became much more pronounced in the later convulsions. The author contents himself with recording these facts, and makes no attempt to speculate as to their probable significance.

The paper is illustrated by tracings obtained of the muscular contractions resulting from excitation of the cerebral cortex with the induced current, and from the convulsions evoked by the intravenous injection of absinthe, and demonstrate the points alluded to in that part of the text which deals with these phenomena.

"The Menstruation and Ovulation of *Macacus rhesus*." By
WALTER HEAPE, M.A., Trinity College, Cambridge. Com-
municated by Dr. M. FOSTER, Sec. R.S. Received June
15,—Read June 18, 1896.

(Abstract.)

The specimens used in the following investigation were collected in Calcutta in 1891.

Anatomy of the Cervix.—A valve-like structure is formed in the canal of the cervix by means of three strong folds, one of these folds fits into a recess formed by the two other folds, and forms a valve which persists throughout life. It is unlike any other structure of the cervix with which I am acquainted.

Breeding.—A definite breeding season for *Macacus rhesus* seems to be proved, but it is equally certain that in different parts of the Continent of India the breeding season occurs at different times of the year.

Menstruation.—A congestion of the skin of the abdomen, legs, and tail, a swelling and congestion of the nipples and vulva, and flushing of the face, are all prominent external signs of menstruation. A regular menstrual flow occurs consisting of a viscid, stringy, opaque white fluid filled with granules, and containing also red blood corpuscles, pieces of uterine tissue, both stroma and epithelium, and also leucocytes.

The following classification of the various stages passed through is adopted :—

- | | |
|----------------------------|---|
| A. Period of rest. | Stage I. The resting stage. |
| B. Period of growth. | Stage II. The growth of stroma. |
| | Stage III. The growth of vessels. |
| C. Period of degeneration. | Stage IV. The breaking down of vessels. |
| | Stage V. The formation of lacunæ. |
| | Stage VI. The rupture of lacunæ. |
| | Stage VII. The formation of the menstrual clot. |
| D. Period of recuperation. | Stage VIII. The recuperation stage. |

The surface of the uterine mucosa, which is smooth and semi-transparent during Stage I, becomes swollen and opaque during Stage II, and flushed during Stage III; it then becomes highly congested, Stage IV, and dark red spots, due to the formation of lacunæ, appear on the surface in Stage V; when Stage VI is reached, free

blood is found in the uterine cavity; the menstrual clot is formed during Stage VII, and the torn mucosa is healed in the final, Stage VIII.

Histology.—The uterus consists of an internal mucosa and external muscular layers; the mucosa is composed of uterine and glandular epithelium, blood vessels, and stroma. The uterine epithelium lines the surface of the stroma, the glandular epithelium lines pits in the stroma and is continued into branches of those pits which extend from their lower end into the deeper part of the stroma.

The stroma itself is a delicate connective-tissue-like layer; the internuclear protoplasm is drawn out into delicate processes which form a continuous network, and there is no intercellular substance.

The histological changes which take place during the menstruation of *Macacus rhesus* are very similar to those which I have already described in a former paper, "The Menstruation of *Semnopithecus entellus* ('Roy. Soc. Proc.,' vol. 54, and 'Philosophical Transactions,' vol. 185). Work similar to that which I have already described for *S. entellus* has been undertaken for *Macacus rhesus*, and the phenomena compared step by step. While it has been thought advisable to note the points of similarity and of difference which occur in the menstruation of these two species, and to point out the fact that the results arrived at by the study of the menstruation of *Macacus rhesus* entirely confirm the results which my examination of *S. entellus* led me to publish, I have purposely avoided all unnecessary repetition and have been obliged in consequence to assume some knowledge of the details given in my former papers. It is all the more important to publish this account, as the results which I have arrived at differ in some important particulars from the accounts of menstruation which have been generally accepted.

Stage I.—The mucosa of *Macacus rhesus* is thicker and the protoplasmic network denser, the glands more numerous and more branched than is the case in *S. entellus*. I find no radial fibres.

Stage II.—There is a great increase in the number of nuclei by amitotic division and fragmentation. Hyperplasia occurs. The mucosa becomes much swollen.

Stage III.—The vessels increase in number and size, and they are congested. There is an increase of leucocytes.

Stage IV.—Hypertrophy of the walls of the vessels in the superficial part of the mucosa, followed by degeneration, occurs; the small vessels break down and extravasation of blood takes place. There is no sign of the migration of leucocytes.

Stage V.—Lacunæ are formed at first some distance below the epithelium, but they gradually displace the intervening tissue and come to lie directly below the uterine epithelium.

Stage VI.—The uterine epithelium degenerates and ruptures, and the blood contained in the lacunæ is poured into the uterine cavity.

Stage VII.—Denudation follows, and the formation of the mucosa menstrualis takes place in the same way and to the same extent as in *S. entellus*.

Stage VIII.—The recuperation takes place as in *S. entellus*. With regard to the new uterine epithelium I find fresh evidence in support of my contention that it is formed, not solely from epithelial elements which already exist, such as the torn edges of glands, but also directly from elements of the stroma tissue.

Ovulation in Macacus rhesus.—Only one case has been met with in which it can possibly be supposed that ovulation and menstruation have occurred simultaneously; this is the only case in which a recently discharged follicle was found in the ovary of a menstruating *Macacus rhesus*; it does not follow that ovulation in this case was brought about by menstruation; indeed, the absence of any sign of the recent bursting of a follicle in any other of the seventeen cases examined is in itself strong presumptive evidence that the two processes are distinct.

This result may be confidently asserted for *Macacus rhesus* during the non-breeding season; at the same time it must be remembered that I have not investigated *Macacus rhesus* during the pairing season; probably at that time ovulation may be more frequent, and may more often be coincident with menstruation; but, however that may be, menstruation occurs in *Macacus rhesus* regularly without ovulation taking place, and my former views are confirmed, namely, that ovulation does not necessarily occur during each menstrual period, and that it is not necessarily brought about by menstruation.

I feel warranted in going further than this and asserting that the regular occurrence of menstruation without ovulation, even though it be in the non-breeding season, is sufficient evidence that ovulation is a distinct process, and that it depends upon a law or laws other than the laws which govern menstruation.

The Discharged Follicle.—The changes undergone by the discharged follicles of *Macacus rhesus* during the non-breeding season are of interest. Very shortly after rupture the follicle is pear-shaped, and the place where rupture took place is to be seen in sections.

The wall of the follicle is composed of branched cells which, along the inner edge of the follicle, are longitudinally disposed and form a denser layer sharply defining the wall from the central cavity.

The cavity contains a network of densely granular material and no blood clot.

Hypertrophy now takes place, the wall becomes much thickened and folded, and a growth of cells takes place from the wall into the cavity of the follicle, the sharply marked boundary of the wall is lost, and the long protoplasmic processes of the cells within the cavity are continuous with the cells of the wall.

The vessels of the wall now become enlarged and increased in number. Hypertrophy is no longer evident; the tissue is denser and shrunk, and the whole structure is reduced in size. Gradually the cavity of the follicle is also reduced in size, and the tissue contained therein becomes denser until it is hardly to be distinguished from that composing the wall.

Finally the whole of the cellular remains of the follicle consist of a comparatively small mass of cells with no trace of the follicle wall and no central cavity, a nearly solid mass of tissue, in the midst of which a few blood vessels run. The cells which compose this mass now scarcely differ from the ovarian stroma cells; they have gradually undergone the change, and instead of branched cells they now appear as polyhedral cells or multinucleated polyhedral protoplasmic masses with intermediate finely branched connective tissue elements bounding them.

This structure is surrounded by a layer of fine nucleated fibres; but soon these disappear, and the remains of the follicle are no longer distinguishable from the rest of the ovarian stroma.

Throughout, no trace of a blood clot within the follicle was seen, and therein these ruptured follicles differ from what is usually described as a normal ruptured follicle in the human female. This difference between two animals, both of which undergo menstruation, is remarkable and worthy of special attention.

I have some reason to believe the difference may be due to the presence or absence of the breeding season in monkeys, and to periods in the human female, which are in the one case favourable, and in the other case not favourable, to conception.

If this be true, the period of the human female which is unfavourable to conception would be comparable to the non-breeding season of monkeys, and the period favourable to conception with the breeding season of monkeys.

It is not maintained that among civilised peoples at the present day there are definite breeding and non-breeding times, but the comparison is in harmony with the view that at one period of its existence the human species had a special breeding season.

“The Homogeneity of Helium and of Argon.” By WILLIAM RAMSAY, Ph.D., F.R.S., and J. NORMAN COLLIE, Ph.D., F.R.S. Received July 21, 1896.

Preliminary.

It was pointed out by Lord Rayleigh and one of the authors that it is a legitimate conclusion to draw, from the found ratio between its specific heat at constant pressure and that at constant volume, that argon is a monatomic element (*Phil. Trans.*, 1895, A, p. 235). A similar deduction can be drawn regarding helium (*Chem. Soc. Trans.*, 1895, p. 699). And as the molecular weight of hydrogen is accepted as twice its atomic weight, and as the density of helium is approximately 2, and that of argon approximately 20, the molecular weights of these elements are approximately 4 and 40 respectively. If, however, the molecule is identical with the atom, then the atomic weights must also necessarily be 4 and 40.

But argon, with an atomic weight of 40, finds no place in the periodic table of the elements, if, as is usual, it is contended that the elements must necessarily follow each other in the numerical order of their atomic weights.

Certain suppositions may be made which would obviate this difficulty. First, the evidence from the ratio of the specific heats may lead to a false conclusion. But it is inconceivable that any structure, except one of the simplest kind, should transform all energy communicated to it as heat, into kinetic energy of translation. Still, before a final decision on this point is arrived at, it would be well to actually determine the specific heat of argon, and this will shortly be done. It may, however, be mentioned, that preliminary experiments have shown it to be much lower than that of hydrogen, air, or carbon dioxide, volume for volume.

Second, helium and argon may consist of a mixture of monatomic with diatomic molecules. The perfectly normal expansion of these gases appears to negative this supposition (*Phil. Trans.*, *loc. cit.*, p. 239, and *Roy. Soc. Proc.*, vol. 59, p. 60). Even at a temperature of -88° there appears to be no marked tendency towards association. It is true that the ratios of the specific heats do not quite reach the theoretical number 1.667. That found for helium was 1.652, and that for argon 1.659, with the most carefully purified samples. Assuming (what there seems good ground to doubt) that the last decimal place may be trusted, helium can be calculated to contain nearly 7 per cent. of diatomic molecules, and argon rather more than 3 per cent. If this calculation be permitted, the atomic weight of helium would become 4.02, taking its found density at

2·15, and of argon 38·62. This would place argon below potassium, the atomic weight of which is 39·1. However, it must be acknowledged that such refinements in calculation are far from trustworthy.

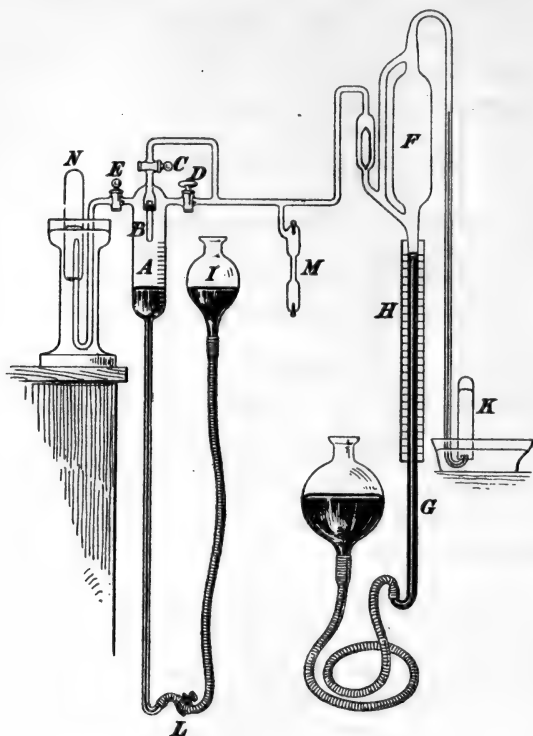
Third, helium and argon may each consist of a mixture of two or more elements. This view has been expressed with regard to helium by Professors Runge and Paschen ('Sitzungsber. d. Akad. d. Wissensch.,' Berlin, 1895, pp. 639 and 759), on the ground that the lines of its spectrum can be shown to belong to two distinct series. The question whether argon is a mixture or not was discussed in the memoir by Lord Rayleigh and one of the authors (*loc. cit.*, p. 236). It is with this possibility that the present communication has to deal.

Two methods suggest themselves as suitable in order to ascertain whether argon and helium are mixtures of two or more elements, or are single elements. The first is fractional solution in water; the second fractional diffusion. The second method is obviously the better calculated to yield the desired data; for if these gases contain constituents of different density, diffusion is an infallible means of separating them.

Description of Diffusion Apparatus.

After a number of trials, the stem of an ordinary tobacco-pipe was found to yield the best results. Plaster of Paris is too porous, and various forms of graphite tried did not effect so rapid a separation of two known gases as unglazed clay. In fact, nothing could have been more satisfactory than this apparatus.

It consists of a reservoir for the gas, A, into which projects a piece of the stem of a tobacco-pipe, B, sealed at the lower end in the flame of an oxy-hydrogen blowpipe. When the stop-cock C is open, and D and E shut, the gas in A must pass through the pipe-clay tube on its way to the reservoir of the pump F. The fall of the mercury in the tube G, read on the scale H, is timed, about 8 cm. fall being taken as sufficient for the purpose. The mercury rises in A, and falls in the reservoir I during the diffusion. When the experiment is finished, the gas is pumped out of the reservoir F, and collected in tubes similar to that depicted at K, and stored in a frame resembling a miniature umbrella-stand. The stop-cock D is then opened, and the clip L is shut, and the less diffusible portion of the gas is pumped out and collected in other tubes, and set apart. The purity of the gas is ascertained by means of the vacuum tube M. After all gas has been removed, the stop-cocks C and D are shut; a new charge of gas is introduced at N, the stop-cock E being opened, and the operation repeated. After a sufficient amount of the first diffusate has been collected, it is again introduced into the reservoir A, and the process repeated.



When towards the end only a small amount of gas is available, the process may be modified by raising the reservoir I, and so diminishing the volume of A. The clip L is then closed, and the gas is allowed to diffuse as before, but the volume in A is kept constant. The rate of diffusion can be compared with that of hydrogen under precisely similar circumstances.

In all the experiments the temperature did not alter by more than a degree or two; as the object was to effect a separation, and not to make accurate determinations of the rates of diffusion of gases, careful regulation of temperature was unnecessary.

Determination of the Ratios of Diffusion of Gases of known Purity.

- (a) *Hydrogen*.—The time required for the column of mercury in H to sink through 8 centimetres, starting always from the same level, was found in three experiments to be (1) 433", (2) 420", and (3) 437"; the mean is 430". The average rate per millimetre is 5·37".
- (b) *Oxygen*.—The time which pure oxygen, made from permanga-

nate, took to diffuse to the same extent was 1719", giving an average rate per millimetre of 21·49".

- (c) *Acetylene*.—The gas was prepared from pure calcium carbide by the action of water. It dissolved completely in alcohol. The time required for diffusion was 1550", giving a rate per millimetre of 19·37".

Assuming the times for the diffusion of these gases to be proportional to the square roots of their densities, we have—

$$\text{For oxygen } \frac{5\cdot37'' \times \sqrt{16}}{\sqrt{1\cdot0082}} = 21\cdot39''. \quad \text{Found } 21\cdot49''.$$

$$\text{For acetylene } \frac{5\cdot37'' \times \sqrt{13\cdot008}}{\sqrt{1\cdot0082}} = 19\cdot29''. \quad \text{Found } 19\cdot37''.$$

This process may therefore be trusted to give fairly accurate results when applied to test the rates of diffusion of gases of known purity.

The Separation of a Mixture of Gases.

To ascertain whether a separation could be easily effected, experiments were made (a) on a mixture of oxygen and carbon dioxide, and (b) on a mixture of hydrogen and helium.

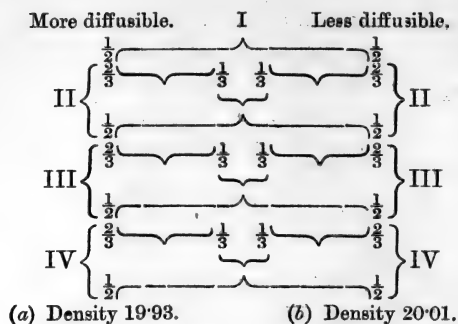
(a) *Oxygen and Carbon Dioxide*.—The original mixture contained 36 per cent. by volume of carbon dioxide. It was split into two approximately equal portions; each of these was again split into two. The most diffusible part contained 30·2 per cent. of carbon dioxide, and the least diffusible part 41·0 per cent.

(b) *Hydrogen and Helium*.—The original mixture contained 50 per cent. of each gas, and its volume was 38 c.c. 19 c.c. were diffused; this was again halved, 9·5 c.c. being passed through the pipe; and finally another diffusion of the 9·5 c.c. yielded 4·12 c.c. of mixed gases. The hydrogen was removed by explosion with oxygen. This mixture now consisted of 67 per cent. of hydrogen and 33 per cent. of helium.

From these experiments it is seen that a partial separation of such gases is easily carried out.

The Fractional Diffusion of Argon.

Four hundred c.c. of argon, newly circulated over red-hot magnesium until spectroscopic traces of nitrogen were carefully removed, was diffused according to the subjoined scheme:—



The densities were determined by weighing.

These numbers show that no important separation has been effected. The difference in density of the two portions may possibly be attributed to experimental error. When the density of the heavier portion was taken the weather was damp, and we have found it difficult to obtain concordant results under such circumstances, owing doubtless to the uneven deposition of moisture on the surfaces of the bulb and its counterpoise. But as it stands, the difference is an extremely minute one, and it may, we think, be taken that any separation of argon, if effected at all, is very imperfect.

The Fractional Diffusion of Helium.

Two hundred c.c. of helium from fergusonite of density 2.13 were separated into two nearly equal portions by diffusion. The rate of diffusion was 7.14" per millimetre as a mean of two experiments, giving 7.13" and 7.15" respectively. The most diffusible portion of this gas gave the rate 7.12" per millimetre. The more diffusible half of this gas had the rate 7.48", and the least diffusible of the remainder 7.38", the temperature being lower. A second specimen of helium from mixed sources, samarskite, fergusonite, bröggerite, &c., which showed the nitrogen spectrum strongly, gave a rate for the first portion of 8.29". This half on rediffusion had the rate 7.64", and the residue of 8.39", showing that a separation was being effected. The heavier residue of the remainder from that portion which showed the rate 8.39" was too small to make it possible to diffuse it by the usual method. A second method was therefore resorted to, and it was directly compared with hydrogen under the same circumstances. While hydrogen took 12.14" per millimetre, the residue took 21.00", and calculating its density from these rates, we have—

$$\frac{21.00''^3 \times 1.0082}{(12.14'')^2} = 3.02.$$

This would correspond, if it be granted that the impurity is nitrogen, to a percentage of 8.5 of that gas. This residue showed a

strong nitrogen spectrum ; and the nitrogen was removed by sparking with oxygen in presence of soda, until the spectrum attested its absence. (It will be remembered that 0·01 per cent. of nitrogen is still visible under moderate pressures, 'Roy. Soc. Proc.,' vol. 59, p. 265.) The rate was again measured against that of hydrogen under precisely similar conditions, and it was found that while hydrogen took 20·00" for diffusion, this specimen of helium took 28·28". And calculation shows its density to be now 2·015.

These experiments were sufficient to show, we think, that while it is possible to separate nitrogen from helium, even although the former is present in only small amount, we had not succeeded in separating helium itself into two portions of different densities. If, then, helium were a mixture, its constituents must possess nearly the same density. In no case was any alteration of the spectrum to be noticed ; the diffusate and the residue were similar, and showed all the well known lines of helium with the usual intensity.

But it was deemed advisable, in view of the importance of the matter, to undertake a much more elaborate set of experiments. The helium was carefully purified from hydrogen and nitrogen by circulation over magnesium, copper oxide, phosphorus pentoxide, and soda lime, until a small quantity admitted into a vacuum tube in connection with the circulating apparatus showed no spectrum either of hydrogen or nitrogen, even at a comparatively high pressure, when these gases are more easily detected. The helium was then fractionated in a manner exactly similar to that shown in the graphic scheme for argon (p. 210). The rates of diffusion of the two samples of gas were then measured.

More diffusible portion—

Time of diffusion reduced to 0°.....	662·5"
Hydrogen	492·3"
Density, calculated from rate	1·826

Less diffusible portion—

Time of diffusion	654·9"
Hydrogen, at same temperature	484·4"
Density, calculated from rate	1·842

The density of hydrogen was taken as 1·0082, on the standard, oxygen = 16.

These samples were next weighed.

More diffusible portion—

Volume of globe.....	162·843 c.c.
Pressure at filling	668·5 mm.
Temperature	19·20°
Weight	0·02450 gram
Density	2·049

Less diffusible portion—

Volume of globe.....	162·843 c.c.
Pressure at filling	663·8 mm.
Temperature	19·93°
Weight	0·02902 gram
Density	2·452

The less diffusible portion was next subjected to the process of removing nine-tenths, the remaining tenth being collected apart. This process was repeated three times, so that any portion of gas less diffusible than the main bulk should thus be left as a residue. From the more diffusible portion nine-tenths was also diffused out. The more diffusible portions were then mixed, and the density was again determined.

Volume of globe.....	162·843 c.c.
Pressure at filling	765·7 mm.
Temperature	20·98°
Weight	0·02801 gram
Density	2·057

This number is practically identical with that previously obtained, viz., 2·049.

It was of interest to follow the less diffusible gas, so as to ascertain what impurity caused its higher density. Another set of fractionations was therefore carried out, and after five separate processes, in each of which a residue was left, and that residue further diffused, so as to separate all light gas as completely as possible, a few c.c. of gas were collected, in which the spectrum of argon was strong. Now we are certain that at no stage in the operations was any considerable quantity of air admitted by leakage. It may safely be said that the total amount of air could never have exceeded 5 c.c. And inasmuch as the density of samples of helium from various sources, which had undergone very little handling, differed by small amounts, varying between 2·114 and 2·181, this must be ascribed to contamination with argon, contained in the mineral from which the helium had been obtained. Every effort was made to detect any unknown lines in the spectrum of the residue, but in vain. With the jar and spark-gap, the blue spectrum of argon was visible, and was compared with that from a standard tube.

If thus the increased density is due to argon, it is possible to calculate the proportion of the latter; first, in the lightest gas of density 2·117 found in samarskite; second, in the residue in which the argon had been concentrated, possessing the density 2·452, on the assumption that helium possesses the density 2·042. The first must contain 0·42 per cent. of argon; the second, 2·28 per cent.

The rate of diffusion of the gas of density 2·057 was determined finally, so as to afford a check on its density. It took 657·9" for a quantity to diffuse; while the same volume of hydrogen under precisely similar circumstances took 492·3". Reducing these numbers to density, if hydrogen be taken as 1·0082, the helium possesses the density 1·801, which compares very favourably with the number already found, 1·826.

As a final check on these results, a sample of helium from an entirely different source, samarskite, was so diffused, that first nine-tenths were removed by diffusion; from the residue nine-tenths was again removed, and the process was repeated a third time. The more diffusible portion was tested as regards rate; while hydrogen took 492·3" to diffuse, this sample took 652·6". Stated as density, the number is 1·771.

The actual density was next determined, with the following result:—

Volume of globe.....	162·843 c.c.
Pressure at filling	691·6 mm.
Temperature	19·85°
Weight	0·02567 gram
Density	2·080

This number closely coincides with the density of the previous specimen, freed from argon by diffusion; and in this case it must be remembered, no systematic process for separating two possible constituents was carried out, but the heavier portion only was removed. The heavier gas separated by diffusion was examined for argon, and it was possible to see the green group of five lines, but not the red lines. And with a jar and spark-gap, argon could just be detected.

The rate of diffusion of this gas, which, stated as density, gives the number 1·8, differs from the density determined by weighing, viz., 2·08, or thereabouts. This might be caused (1) by a lighter portion passing over first during diffusion, leaving a heavier portion behind; or (2) by the hypothesis that the rate of diffusion of helium is abnormal; and helium has already shown such very remarkable properties in relation to refractivity for light, and conductivity for electricity, that the hypothesis is not unwarrantable. The first supposition, however, is the more probable, and was put to the test in the following manner.

A smaller apparatus was made for measuring the rate of diffusion of 10 to 20 c.c. of gas; and the rates of the sample of density 2·08, and of the less diffusible residues from this sample were determined. Both the hydrogen and the helium were carefully measured and diffused under precisely similar conditions. While the hydrogen took 181" to diffuse, the helium of density 2·08 took 246·6", implying a

density of 1·871; and the residue diffused in 266·6", which corresponds to a density of 2·187. In each of these experiments about half the helium passed through the porous plug.

The denser portion of this gas was again diffused five times, lighter portions being removed. This corresponds to a residue of 30 c.c. from 400 c.c. of the original gas. The rate of diffusion of this sample compared with that of hydrogen was almost identical with the last, namely 208" to 143", and corresponds to a density of 2·133. The gas is therefore not increased in density by this process.

The lighter gas was submitted to a similar fractionation, and the ratio of its diffusion-rate to that of hydrogen was 246·75" to 181·0", as a mean of several closely concordant experiments. This corresponds to a density of 1·874. We have accordingly:—

	Density.
"Heavy" portion.....	2·133
"Light" portion.....	1·874

Not content with this, we pushed fractionation still further; the helium was divided into seven portions (by fractionation) and then submitted to methodical fractional diffusion, in which the heavier portions were transferred to the "denser" side, and the lighter portions to the "lighter" side. This process was repeated four times, and the end portions were each divided into two; the lighter portion of the "lighter" was collected separately, and its rate determined. It took 258·5" to diffuse, compared with 189·5" for an equal volume of hydrogen; its density calculated from these rates was 1·876. It is clear, therefore, that the limit has been reached in purifying the lighter portion by diffusion.

It should have been mentioned that the portion of 2·133 density as well as that of 1·874 density had been sparked with oxygen in presence of potash, and in a vacuum tube showed mere traces of hydrogen, every other gas being absent. The spectrum of hydrogen is still visible, even when 0·01 per cent. of that gas is present.

At various times during the attempt to separate helium, the spectrum has been carefully examined. *The very first* portions of the lightest gas gave an identical spectrum, seen with a hand-spectroscope, with *the very last* portions of the heaviest gas. Professor Ames, of the Johns Hopkins University, has however kindly undertaken to photograph the spectra using a dispersion-grating; so that if any difference can be detected, it will ere long be made known.

Lord Rayleigh was so kind as to measure the refractivity of these extreme portions of the fractionated gas. His process has been described in the 'Proceedings,' vol. 59, p. 202. For the sample of helium sent him in July, 1895, he found the number 0·146. The lighter portion of the fractionated gas of density 1·876 had a refrac-

tivity, compared with air as unity, of 0.1350; the heavier portion, of 0.1524. The ratio of these numbers is very nearly that between the densities of the gases, viz. :—

$$\frac{0.1350}{0.1524} = \frac{1.876}{2.118}, \text{ instead of } \frac{1.876}{2.133}.$$

Conclusion.

It must be remarked that the rate of diffusion of helium is too rapid for its density measured by weighing. There can be no doubt, we think, that the density of the lighter portion, instead of being 1.874, would be, if actually weighed, 2.05 or 2.08. And the heavier portion has doubtless a proportionately higher density. But, assuming that the densities calculated from the diffusion-rates are correct, the densities of the two gases, supposing that two exist, are 1.871. and 2.133, respectively.

Also, we must not omit to state that careful experiments were made with the more rapidly diffusing gas to prove that the first portions passing over did not diffuse at a more rapid rate than the later portions, no difference in diffusion rates, compared with those of hydrogen under the same circumstances having been detected.

That helium, then, consists of a mixture of two or more distinct gases is one solution of the problem, probably the one which recommends itself at first sight. But there is another, so revolutionary in its character that much must be done before it can be regarded as even worthy to be entertained. So much has, however, been lost to science by what may be termed scientific incredulity, that we regard it as well worth putting to a rigorous proof.

It is that a separation has been effected of light molecules from heavy molecules; that, in fact, a gas—in this case helium—is not constituted entirely of molecules of the same weight, but that the mixture of molecules which we term helium have weights which average 2.18, or whatever the density of ordinary undiffused helium may ultimately be found to be. The same supposition would, of course, be applicable to oxygen, nitrogen, or any gas. In separating such molecules from each other a practical limit must necessarily be reached, and this limit appears to have been reached with helium.

There is negative and positive probability in favour of this suggestion. First, no gas has been submitted to methodical diffusion with a view to effect such a separation, argon excepted; and here, too, there is faint evidence of a similar kind. It is proposed to carry out similar experiments with gases of undoubted homogeneity according to the usual views; and till such experiments have been made, it is impossible to decide the point definitely.

Second, Mr. E. C. C. Baly's experiments on oxygen appear to

point to a similar conclusion; although no great alteration in density has been produced, yet there is a sign that a kind of separation is being effected electrically. There is also in favour of the supposition the unlikelihood that two or more gases, so like one another as the constituents of helium, should exist with densities so near each other; and the probability that some separation should have been detected by aid of the spectroscope.

Lastly, the refractivities of both gases, if there be two, appear to be equally abnormal; now, different gases have different refractivities in no known relation to their densities, as, for example, hydrogen 0.5, oxygen nearly 1. But the refractivities of the different portions of helium are proportional to their densities; a statement which is true of any one gas, inasmuch as refractivity is directly proportional to pressure, *i.e.*, mass in unit volume. The refractivity of helium, also, is so small that it totally differs in this respect, as, indeed, it does in most of its physical properties from every other gas, and it is moreover a monatomic gas. It is therefore permissible to seek for an explanation of its remarkable properties in framing any hypothesis which admits of being put to the test.

“On the Spectrum of Cyanogen as produced and modified by Spark Discharges.” By W. N. HARTLEY, F.R.S., Royal College of Science, Dublin. Received July 13, 1896.

The Production of Cyanogen in the Electric Arc.—The very careful and numerous experiments of Liveing and Dewar* have very generally been accepted as affording evidence sufficient to establish the existence of an emission spectrum of cyanogen as distinct from that of carbon in the electric arc. Kayser and Runge,† though at first disinclined to accept such a conclusion, obtained additional evidence by experimenting with the arc in air, and in carbon dioxide. They found that the ordinary carbon spectrum and that of cyanogen appeared with rapidity alternately in the arc in air, though there could be no difference in temperature sufficient to account for the production of two different carbon spectra. With the poles immersed in carbon dioxide no such changes were seen, the carbon spectrum alone being visible, which evidence led them to concur in the views of Liveing and Dewar. The chief evidence of the existence of a cyanogen spectrum rests on the fact that this substance is actually synthesised in the arc when nitrogen is present, and because without

* ‘Roy. Soc. Proc.’ vol. 30, pp. 152—162, 494—509: vol. 34, pp. 123—130 and pp. 418—429.

† “Ueber die Spectren der Elemente. Zweiter Abschnitt. Ueber die im galvanischen Lichtbogen auftretenden Bandenspectren der Kohle.” ‘Abh. K. Preuss. Ak. Wiss.’ 1889, p. 9.

nitrogen, elementary carbon does not yield the same spectrum, no matter what the temperature may be; and lastly, that cyanogen gas burns with a flame of which the banded spectrum is known as that of cyanogen by reason of the foregoing facts. Furthermore, I have found by recent experiments that when a condensed spark is passed between electrodes of gold in an atmosphere of cyanogen, the same spectrum is photographed.

If we admit that under conditions favourable to synthesis from its elements, cyanogen is capable of emitting a spectrum of its own, this emission should occur only at the moment of its formation, but while giving consideration to this view we are met by the difficulty that the flame of cyanogen burning in oxygen would less probably emit a spectrum of the compound substance itself, which is being burnt, than a spectrum of the products of its combustion, or of the separated elements of which it is composed, which are nitrogen and carbon; and for this reason, that the process it is passing through is not a synthetical but an analytical one. Indeed it has been shown by Liveing and Dewar* that when cyanogen is exploded with oxygen it gives a bright continuous spectrum, but no cyanogen spectrum, or carbon bands, or carbon lines.

I shall have to refer to these facts and adduce later evidence of the existence of the cyanogen spectrum in the latter part of this paper.

Evidence derived from their Spectra, of the progress of Chemical Changes in Flames.—In support of the view that the flame of burning cyanogen ought to exhibit the spectrum of carbon, I may mention the following facts which have been recorded during a very careful examination of a number of photographs of the spectra of flames which were obtained by burning gases under normal atmospheric conditions.

The majority of these photographs were taken in 1882.

<i>The Combustion of Compound Substances.</i>	<i>Components of the Spectra photographed.</i>
Hydrocarbons in oxygen.	Carbon bands, cyanogen bands, water-vapour lines.†
Sulphuretted hydrogen in air and in oxygen.	Sulphur bands and water-vapour lines.
Ammonia in air.	Water-vapour lines.
Carbon disulphide in air.	Sulphur bands only.
Carbon disulphide and nitric oxide.	Sulphur bands only.
Carbon monoxide and oxygen.	Continuous spectrum of carbon monoxide. Faint lines due to carbon, very few in number.

* 'Roy. Soc. Proc.' vol. 49, p. 222. "On the Influence of Pressure on Flames."

† When nitrogen is present, Liveing and Dewar have observed the formation of NO_2 (*loc. cit.*).

By the combustion of ammonia in oxygen, water vapour lines are produced, and new bands and groups of lines attributed by Eder and Valenta to ammonia. Some of these are, however, due to a compound other than ammonia.

It will be observed that compounds during combustion as a rule show the spectra of one or other of their constituents, or of both. In the case of hydrogen compounds they show the product of the combustion of hydrogen, which is a substance of great stability, and can therefore exist at a high temperature.

In the nitric oxide and carbon disulphide spectrum, the sulphur bands, which are very strong, probably obscure those of carbon. There is a strong continuous band of rays which would likewise serve to obscure them.

C. Bohn* has examined the spectra seen in a Bunsen burner of the form devised by Tecluf (which is simply a modification of that described by Smithells), and compared the spectra with that obtained by Swan, and with the discharge in Geissler tubes containing various hydrocarbon gases. He concludes that it is impossible to define a carbon band spectrum, as the differences observed were greater than could be accounted for by alterations in temperature and pressure. He also states that sulphur, hydrogen, and carbon disulphide, also carbon monoxide, were burnt, but that all these flames yielded continuous spectra. This statement is incorrect, or at least inaccurate.† Bohn's observations were evidently made on too limited a region of the spectrum, and without the aid of photography. On Bohn's paper Eder has made some observations, quoting both his measurements in the visible and ultra-violet spectrum, which he observes must have been unknown to Bohn.‡

He describes in what manner and by what causes the edges of the carbon bands are altered in position or in character.

The observations of Eder on the spectra of hydrocarbon flames are quite in agreement with those previously communicated by me to the Royal Society on the oxyhydrogen flame spectrum and the oxy-coal gas spectrum.

On certain Chemical Changes occurring in the Spark and in Flames.

Though it is now accepted as a fact that the arc in air yields the spectrum of cyanogen, and that the evidence of this is, first, the identity of certain bands observed in the flame of burning cyanogen

* 'Zeitschrift für physikal. Chemie,' vol. 18, p. 219, 1895.

† 'J. prak. Chemie' [2], vol. 52, pp. 145—160, 1895.

‡ "Flame Spectra at High Temperatures" ('Phil. Trans.,' A, vol. 185, pp. 161—212, 1894).

§ 'Ueber Flammen und leuchtende Gase' ('Zeitschrift für physikal. Chemie,' vol. 19, p. 1, 1896).

with those seen in the arc; second, that these bands cannot be due to the effect of an alteration in temperature, giving rise to a second spectrum of carbon; nevertheless, as I have elsewhere pointed out,* cyanides in a condensed spark do not produce this spectrum, no matter whether they are extremely stable cyanides, such as that of potassium, or those of the most easily decomposable character, such as mercuric cyanide. This appeared to me to mark the inadequacy of the facts derived solely from observations on the arc, to establish the existence of a definite cyanogen spectrum. Moreover, it was shown that lines somewhat resembling the edges of cyanogen bands are seen when graphite poles are moistened with water and the spark is passed through air; these lines are intensified and developed into bands when the water contains ammonium chloride, calcium chloride, or zinc chloride, and the bands become stronger as the solution used is more concentrated.

If the lines observed are the edges of bands belonging to the cyanogen spectrum, by what means do the chlorides give rise to their production? No one has yet supplied the answer to this question, neither has it been proved that these lines in the spectrum of graphite are the edges of cyanogen bands, though Eder† and Valenta state that they are such because the wave-length measurements are approximately the same.

I believe that I am now able to offer an explanation of the action of the concentrated solutions of chlorides, and to prove in addition, that the bands and lines are really due to cyanogen and not to elementary carbon.

If hydrochloric or any other mineral acid be carefully tested, it is found to contain ammonia. The only ammonia-free acid is sulphurous acid freshly prepared by passing sulphur dioxide gas into water, carefully freed from ammonia and from any possible contamination with it. If from the usual samples of so-called pure mineral acids, salts of calcium or zinc be prepared, the ammonia salt present is not eliminated, but it goes into solution and crystallises out with such calcium or zinc compound, or, if the salt does not crystallise, it remains in solution, and, as a consequence, the salt will show in its solution the effect of a larger proportion of ammonium salt, according to its degree of concentration. Hence if the bands, said to be cyanogen bands, are due to the nitrogen of the ammonia present, the spectrum of the graphite poles will exhibit the bands more strongly, as there is less water in the solution. But this does not account for

* 'Phil. Trans.' vol. 175, p. 49, Part I, 1884, and 'Roy. Soc. Proc.' vol. 55, p. 344, "On Variations observed in the Spectra of Carbon Electrodes, and on the Influence of one Substance on the Spectrum of another."

† 'Wien, Akad. Wiss. Denkschriften,' vol. 60, 1893, "Line Spectrum of Elementary Carbon."

the fact that the spark does not show the cyanogen bands when cyanides are submitted to its action. In this case it is possible that the temperature is too high, and that the cyanogen is decomposed, possibly by oxidation, for there can be no doubt that such condensed sparks are at a higher temperature than that of the arc. We know, too, that several metals are oxidised when volatilised in the spark, if not entirely at least partially.* But by using gold electrodes with the cyanides we do not obtain even a carbon spectrum.

Here again, possibly, the carbon is oxidised, and we know that carbon dioxide in carbonates yields no spectrum of carbon, nor any lines peculiar to carbon dioxide.

I have sought in every direction for a reasonable explanation of that which, up to the present, has proved inexplicable, in order that by working on some hypothesis one might devise a means of putting the matter to experimental proof. This has now been accomplished in the following manner.

An almost saturated solution of pure crystallised potassium cyanide was put into a tube fitted with graphite electrodes in the manner described in a previous communication.†

The apparatus was fitted into a horizontal wooden tube with a window of quartz at one end, and carbon dioxide was passed into the tube until filled. The spark was then passed for five minutes, and again for ten minutes, a photograph being taken of the two spectra. The instrument used gave a dispersion equal to four quartz prisms. A glass tube with a similar window of quartz was fitted with gold electrodes and filled with cyanogen gas, and another spectrum was photographed. A fourth spectrum was then obtained by passing cyanogen into the wooden tube containing the graphite electrodes; after the carbon dioxide had been expelled by air and replaced by cyanogen, the U-tube was filled up with the solution of potassium cyanide. In all four cases the principal group of the cyanogen bands was obtained, but it was not very strong. A flame of cyanogen was then photographed with exposures varying from one to two, five, and ten minutes. A beautiful series of spectra was obtained, and the lines belonging to the edges of bands constituting the principal group were found to coincide exactly with those photographed from the potassium cyanide solution when the spark was passed in an atmosphere of carbon dioxide and in cyanogen, also when the spark was passed between gold electrodes in cyanogen. These appear to be the bands referred to by Eder and Valenta, which were described as *carbon bands*‡ when graphite electrodes were used with the spark

* 'Roy. Soc. Proc.,' vol. 49, p. 448, "On the Physical Characters of the Lines in the Spark Spectra of the Elements."

† 'Phil. Trans.,' vol. 175, p. 49, 1884.

‡ Hartley and Adeny, 'Phil. Trans.,' vol. 175, p. 63, Part I, 1884.

in air. From the modification of their appearance, and the measurements originally made from them, their identity was not quite apparent, although probable.

It thus appears that, with the spark, the cyanogen spectrum is nothing like so strongly marked, as is the case with the flame of the gas, only one group of bands being represented, and that when the spectrum is taken in air the cyanogen does not appear, because in all probability the substance is oxidised.

I have already stated that the formation of cyanogen which yields the characteristic spectrum is a synthetical operation, that compound substances, when burnt in flames, do not, as a rule, emit the spectrum of the compound, but the spectrum of one or more of the elements of which it is composed, or that of one or other of its products of combustion.

How then are we to account for the cyanogen spectrum in the flame of burning cyanogen?

The conditions under which combustion takes place are these: there is an excess of the gas, the temperature of the flame is exceedingly high, and the gas within it is not in contact with a solid substance, hence immediate decomposition does not occur, and the gaseous compound is heated to incandescence.

“Variation in *Portunus depurator*.” By ERNEST WARREN, B.Sc.,
Demonstrator of Zoology at University College, London.
Communicated by W. F. R. WELDON, F.R.S. Received
July 1, 1896.

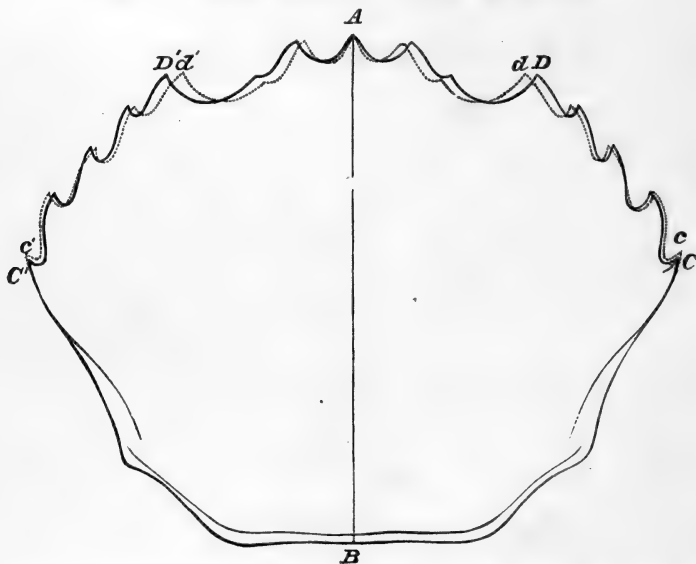
The following measurements were undertaken at the proposal of Professor W. F. R. Weldon, and to him I am greatly indebted for many suggestions, and for the kindly help he has always so readily given me.

The crabs were obtained from the Biological Station at Plymouth, and sent at intervals during a period of about two years, dating from the autumn of 1893. Only males were measured. Seven measurements were made on each individual, corresponding to those made by Professor Weldon on the female of *Carcinus moenas* (‘Roy. Soc. Proc.,’ vol. 54).

1. Carapace length, AB (fig. 1).
2. Total carapace breadth, CC’.
3. Frontal breadth, DD’.
4. Right antero-lateral, AC.
5. Left antero-lateral, AC’.
6. Right dentary margin, CD.
7. Left dentary margin, C’D’.

The total number of crabs measured was 2300. The determinations were made with compasses and a 3-decimetre ivory scale divided into half millimetres. The measurements were recorded to the tenth of a millimetre. As a test of accuracy, fifty crabs were indiscriminately taken out of a large number which had previously been measured; these were remeasured, and the results compared with those before obtained. It was found that the mean difference between any two measures of the same dimension was 0.107 mm.

FIG. 1.—*Portunus depurator* ♂ (Plymouth, 1893-95).



Continuous outline represents a crab with carapace length $AB = 28.5$ mm. Dotted outline represents a crab with carapace length $AB = 46.8$ mm.

Since the crabs were growing, and so varied much in size, it was necessary to reduce the measures to percentages of some standard dimension. The carapace length (AB) was selected, and all the measurements are expressed as thousandths of this dimension.

The first point to ascertain is whether the mean of the different "organs" varied as the crab increased in size. To determine this, the crabs were sorted into groups, according to the length of their standard dimension, then the arithmetic mean was found for the remaining six dimensions. In each group the change in length of carapace was comparatively small, and had to be neglected. The following table gives the results which were thus obtained:—

Grouping according to length of standard dimension.			Total breadth.		Frontal breadth.		R. antero-lateral.		L. antero-lateral.		R. dentary margin.		L. dentary margin.	
Carapace length (AB) in millimetres.	Approximate length corresponding to mean given.	No. of individuals in the group.	* Arithmetic mean.	† Error of mean square.	Arithmetic mean.	Error of mean square.	Arithmetic mean.	Error of mean square.	Arithmetic mean.	Error of mean square.	Arithmetic mean.	Error of mean square.	Arithmetic mean.	Error of mean square.
1	20.0-24.9	17	1291.794	—	765.029	—	784.558	—	785.265	—	463.382	—	466.029	—
2	25.0-31.9	182	1290.874	4.66	728.467	4.12	781.016	3.18	733.280	3.30	470.731	3.73	467.984	3.35
3	32.0-34.9	178	1297.421	5.15	715.163	3.58	781.387	3.08	780.781	3.22	476.264	3.98	475.736	3.41
4	35.0-35.9	164	1300.036	5.34	708.085	3.38	779.378	3.21	777.427	3.11	474.841	3.18	474.232	3.17
5	36.0-36.9	171	1299.120	5.11	704.915	3.55	779.909	3.38	779.091	3.11	479.933	3.68	476.535	3.75
6	37.0-37.9	226	1301.332	5.00	701.353	3.50	780.703	3.03	779.482	2.94	480.385	3.51	479.721	3.22
7	38.0-38.9	234	1300.927	5.06	699.996	3.19	779.055	2.93	778.081	3.04	478.406	3.24	477.850	3.28
8	39.0-39.9	256	1300.297	5.10	695.891	3.15	777.812	2.94	777.031	2.99	478.187	3.54	477.140	3.49
9	40.0-40.9	230	1301.578	4.86	694.474	3.30	778.300	2.79	777.135	2.81	478.526	3.39	477.995	3.34
10	41.0-41.9	217	1303.989	4.97	690.753	3.35	779.767	3.06	778.182	2.99	481.113	3.37	480.288	3.42
11	42.0-42.9	139	1300.011	4.91	688.291	3.45	780.953	3.20	776.694	2.99	480.205	3.63	479.651	3.61
12	43.0-43.9	130	1299.654	5.48	684.054	3.48	777.551	3.57	776.161	3.49	480.884	4.10	480.008	4.10
13	44.0-44.9	84	1301.257	5.29	681.833	3.33	779.166	3.46	777.595	3.15	482.690	4.01	481.738	3.91
14	45.0-48.6	72	1300.611	6.12	675.778	3.97	777.000	3.53	776.111	3.34	484.055	4.40	482.333	4.30

* In thousandths of carapace length.

† in terms of .004 of carapace length.

As an example we may take the total breadth. Here the observed range of variation is from 1370 to 1227 thousandths of the standard. The unit of deviation employed is 0·004 of the carapace length; thus, with this unit the range of deviation in the total breadth is expressed by the number 36. The crabs varied in absolute length of the standard from 20·0—48·6 mm., and in the fourteen groups the means are expressed in thousandths of carapace length, but the errors of mean square and all the constants that will hereafter be given are expressed in terms of the above unit of deviation.

The length of the standard is taken as a criterion of age; this may, of course, be only roughly true, and it seems quite possible that the somewhat large fluctuations which are observed in the means are due to the groups being in reality slightly heterogeneous by including crabs of somewhat different ages.

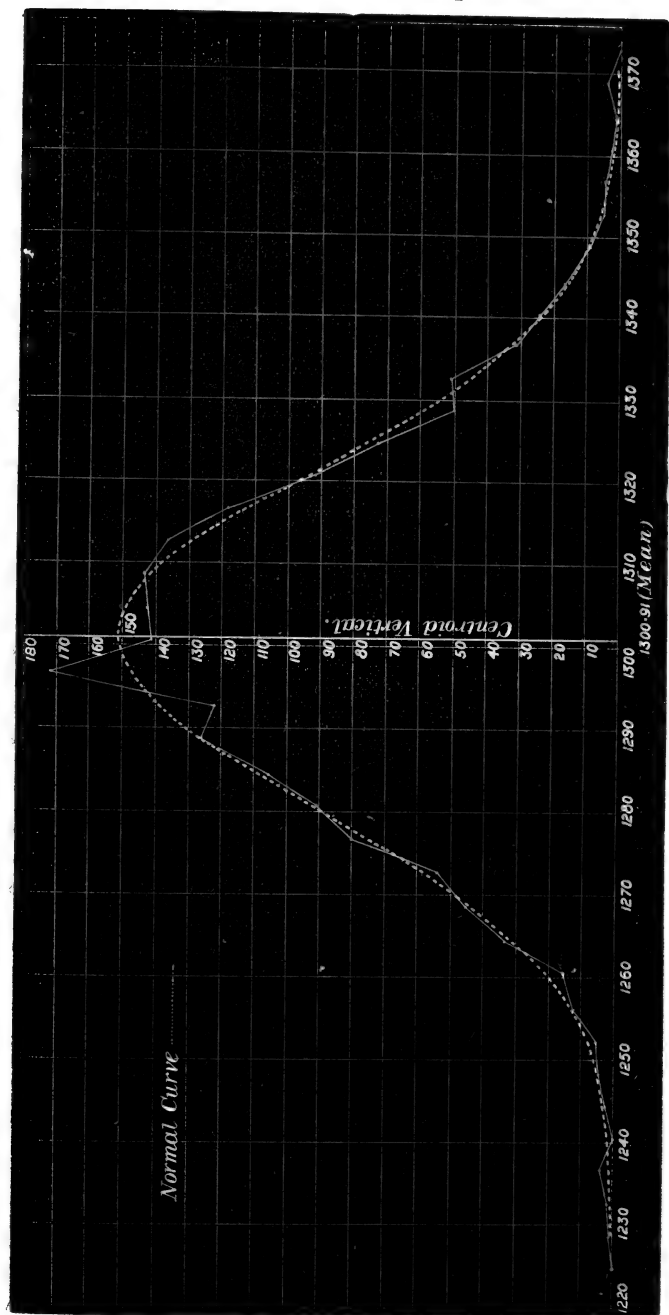
On glancing down the means of the several dimensions, it will be seen that a state of equilibrium is nowhere reached except perhaps in the case of the total breadth; thus, throughout life the crab is gradually changing its shape. This species, like the closely allied *Carcinus moenas*, is probably sexually mature when only some 20—30 mm. long;* hence, when all its organs are in a rapid state of change the crab can propagate its species. Here is an argument, so it would seem, against the transmission of acquired characters, for otherwise the earlier broods would tend to have a somewhat different shape to the later ones, and this is scarcely probable. To illustrate this change in shape fig. 1 was prepared. The continuous outline represents a crab with carapace length = 28·5 mm.; the broken outline is the *same* crab when it has grown to 46·8 mm.; the angle CAC' opens out by about a degree. This, of course, is only true on the supposition that natural selection has not occurred with respect to these dimensions. The error of mean square of each group is given in the columns to the right of the means. This constant we take as a measure of the variability of an organ, and in no case is there a distinct tendency for it to diminish as we pass down to the groups containing the older crabs, while in the dentary margins there is an obvious increase. Hence, at the present period in the life of the species we have no evidence of selective destruction with regard to the dimensions here discussed. It is possible, however, that all the dimensions are really more variable as the crab grows older, but that this greater variability is concealed by the action of natural selection in all cases except in the dentary margins.

We will now treat each organ separately.

Total Breadth.—On referring to the table, it will be seen that groups 4—14 have means which remain fairly steady, and show

* This point is now being investigated. Female *Carcinus* occurs in berry when only some 20 mm. long.

FIG. 2.—Total Breadth, 1923 Individuals.



Horizontal scale represents thousandths of carapace length. Vertical scale represents number of individuals.

no marked tendency either to rise or fall. With these 1923 individuals a curve of frequency was drawn (fig. 2). Its constants were calculated by the method employed by Professor Karl Pearson ('Phil. Trans.,' vol. 186). Range of variation = 1227—1370 thousandths of standard, unit of deviation is 0.004 of carapace length; therefore the observed range = 36 units, reckoning from 1227 upwards.

Centroid vertical (= position of arithmetic mean) = 19.102964.

The second moment about the centroid (μ_2) = 26.400476.

Standard deviation (error of mean square), $\sigma = \sqrt{\mu_2} = 5.138139$.

Third moment (μ_3) = 0.681766.

Fourth moment (μ_4) = 2203.762099.

β_1 , which is $\mu_3^2/\mu_2^3 = 0.000025$; $\beta_2 = \mu_4/\mu_2^2 = 3.161849$.

The critical function $2\beta_2 - 3\beta_1 - 6 = 0.323623$ is positive, and so the theoretical curve has an unlimited range.

Professor Pearson's measure of skewness for a curve of unlimited range is given by the formula

$$\frac{1}{2} \sqrt{\beta_1} \frac{r-2}{r+2} \text{ where } r = \frac{6(\beta_2 - \beta_1 - 1)}{2\beta_2 - 3\beta_1 - 6}.$$

Here $r = 40.080414$ and skewness = 0.002262. It is clear from the values of the constants that the generalised probability curve would not differ perceptibly from the symmetrical normal curve, where $\beta_1 = 0$ and $\beta_2 = 3$.

The areal deviation of the curve of observation from the normal curve is only 5.1 per cent. of the whole area.

Frontal Breadth.—It will be seen from the table that throughout the life of the crab the mean of this dimension falls steadily; as the crab grows the forehead becomes relatively shorter. On this account it is difficult to obtain a satisfactory idea of the distribution of deviations. The means of groups 6—7 do not differ widely, and so with these the constants of variation were calculated. The observed range throughout the whole series was 640—795 thousandths of standard, and so there are 39 of our units of deviation. The range in groups 6—7 (including 460 crabs) was 648—747 thousandths, that is, 25 units.

$$\text{Centroid} = 13.791305.$$

$$\mu_2 = 11.236156.$$

$$\sigma = 3.352036.$$

$$\mu_3 = 2.800794.$$

$$\mu_4 = 442.572048.$$

$$\beta_1 = 0.005529.$$

$$\beta_2 = 3.505490.$$

$$r = 15.084346. \quad \text{Skewness} = 0.02845.$$

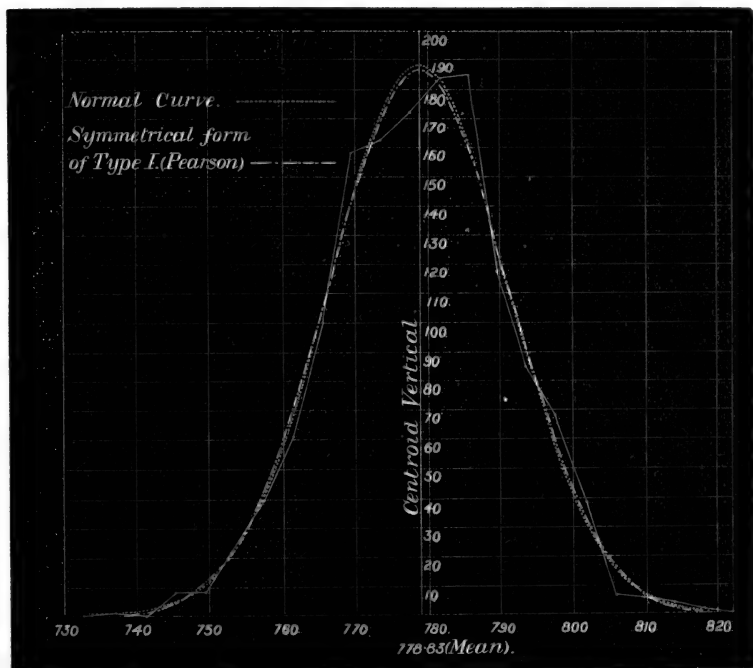
Here again the critical function $2\beta_2 - 3\beta_1 - 6 = 0.994393$ is positive

and so the generalised probability curve would be one of unlimited range.

R. Antero-lateral.—The mean fluctuates considerably in the different groups, but there is a slight tendency to fall. Omitting the first group as being too small, it will be seen the mean falls about one unit in the whole series.

Range of deviation = 732—831 thousandths of standard, giving 25 units of variation. We select groups 6—12 (1432 individuals) for forming a frequency curve. Here the range = 736—819 thousandths, giving 21 units.

FIG. 3.—*R. Antero-lateral*. 1432 Individuals (see fig 2).



$$\text{Centroid} = 11.331704.$$

$$\mu_2 = 9.296165.$$

$$\sigma = 3.048978.$$

$$\mu_3 = -1.075979.$$

$$\mu_4 = 255.659399.$$

$$\beta_1 = 0.001441.$$

$$\beta_2 = 2.958381.$$

$$r = \frac{6(\beta_2 - \beta_1 - 1)}{3\beta_1 - 2\beta_2 + 6} = 134.096687.$$

$$\text{Skewness} = \frac{1}{2} \sqrt{\beta_1 \frac{r+2}{r-2}} = 0.01955.$$

The critical function $2\beta_2 - 3\beta_1 - 6 = -0.087561$ is negative, and so the theoretical curve has a limited range. The calculated range was 71.313123 units, and the observed range 21 units; thus they differed very widely. The range on the positive side of the origin was 31.658658, and on the negative side 39.654465. As the frequency curve is very symmetrical, Professor Pearson's generalised curve with limited range and symmetry was taken,

$$y = y_0 (1 - x^2/a^2)^m,$$

where $y_0 = 186.357143.$ $a = 35.656561.$

$$m = 66.048343.$$

The distance of maximum ordinate from centroid vertical is 0.0596, and this could not be indicated on the scale of diagram. Both this and the normal curve are drawn over the curve of observation (fig. 3). The generalised curve differs exceedingly little from the normal one, the areal deviations in the two cases being 7.7 and 7.5 per cent. respectively.

The flat top to the frequency curve made it at first seem probable that the curve was really the resultant of two normal ones. It was attempted to resolve it into its constituents by means of Professor Pearson's equation of the ninth degree ('Phil. Trans.,' vol. 185). There were three real roots to the equation; two gave quite inappropriate solutions; the third gave a negative group of sixty crabs (with standard deviation = 1.450), having its maximum ordinate situated at -1.337 from the centroid, and a positive group of 1492 crabs (standard deviation = 3.012), with maximum ordinate at -0.0540. The selection of crabs so close to the mean would scarcely seem to correspond to any natural phenomenon, and the resultant curve, which was the difference of the two normal curves, fitted the curve of observation but very little better than the normal or skew curve in fig. 3, the areal deviation being 7.0 per cent.

L. Antero-lateral.—Here the range of the mean is somewhat larger than on the right side, being about $1\frac{1}{2}$ units. The range of deviation in the whole series is 720—823 thousandths. A frequency curve was drawn (fig. 4) with groups 6—12, where the range was 724—819 thousandths, giving 24 units.

$$\text{Centroid} = 14.046787. \quad \mu_2 = 9.127467.$$

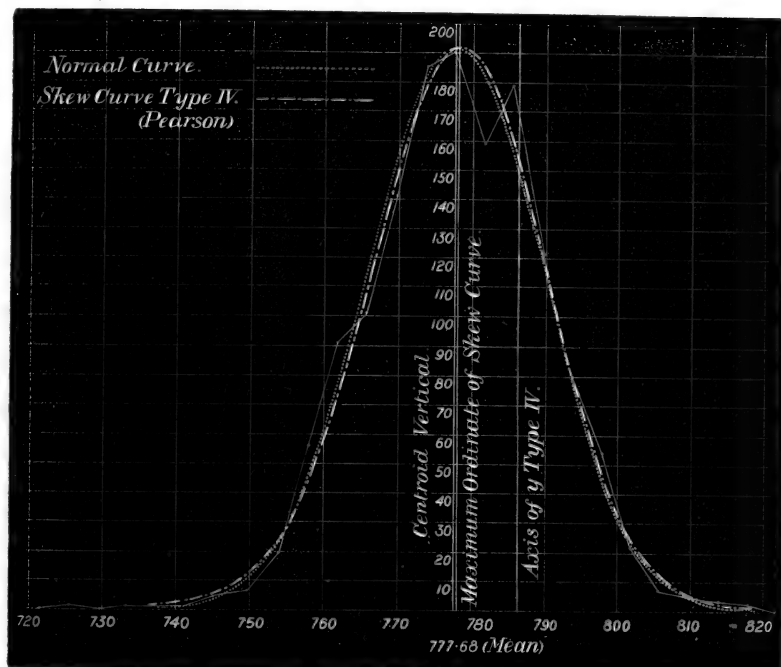
$$\sigma = 3.021170. \quad \mu_3 = -1.885642.$$

$$\mu_4 = 263.267646.$$

$$\beta_1 = 0.004676. \quad \beta_2 = 3.160072.$$

$$r = 42.246651. \quad \text{Skewness} = 0.031099.$$

FIG. 4.—L. Antero-lateral. 1432 Individuals (see fig. 2).



The critical function $2\beta_2 - 3\beta_1 - 6 = 0.306116$ is positive, and so the range is unlimited, as was the case with the total and frontal breadths.

Using Professor Pearson's skew curve of unlimited range, and putting

$$x = a \tan \theta,$$

we have

$$y = y_0 \cos^2 m \theta e^{-v\theta},$$

where

$$y_0 = 152.4817.$$

$$v = 4.550107.$$

$$m = 22.123325.$$

$$a = 19.291468.$$

The distance of axis y from centroid = 2.077756, and the distance of maximum ordinate from centroid = 0.093918. Both this and the normal curve are drawn, and there is but very little difference between them, the areal deviations being 7.5 and 7.9 per cent. respectively.

R. Dentary Margin.—The mean has a range of about $3\frac{1}{2}$ units, and it rises as the crab grows. The total range of deviation = 436—539 thousandths. As in the other cases, groups 6—12 were selected, and a frequency curve drawn; the range is 436—531 thousandths, giving 24 units.

$$\begin{aligned}
 \text{Centroid} &= 11.504888. & \mu_2 &= 12.483682. \\
 \sigma &= 3.533226. & \mu_3 &= 1.936527. \\
 & & \mu_4 &= 451.998740. \\
 \beta_1 &= 0.001927. & \beta_2 &= 2.900359. \\
 r &= 55.546793. & \text{Skewness} &= 0.023589.
 \end{aligned}$$

The critical function $2\beta_2 - 3\beta_1 - 6 = -0.205063$ is negative, and so the theoretical curve has a limited range. This range is 53.325; in the actual statistics it is 24, and so here as in the case of the R. antero-lateral it much exceeds any conceivable limit that may exist for the crab.

L. Dentary Margin.—The trend and range of the mean resemble those of the R. dentary. The total observed range of deviation is 417—524 thousandths. In groups 6—12 the range is 425—524, giving 24 units.

$$\begin{aligned}
 \text{Centroid} &= 14.071229. & \mu_2 &= 12.061035. \\
 \sigma &= 3.472903. & \mu_3 &= -4.649576. \\
 & & \mu_4 &= 438.990665. \\
 \beta_1 &= 0.012322. & \beta_2 &= 3.017770. \\
 r &= 8438.070126. & \text{Skewness} &= 0.055527.
 \end{aligned}$$

The critical function $2\beta_2 - 3\beta_1 - 6 = -0.001426$. From this we see that the theoretical curve has a limited range; but this range would be enormous, and the curve would closely resemble a normal curve.

Correlation of the Organs.—Out of the six organs discussed, the frequency curves of three of them (total breadth, frontal breadth, and L. antero-lateral) give theoretical curves of unlimited range, while the other three (R. antero-lateral, R. and L. dentary margins) give curves of limited range. In every case the amount of skewness is small, and the diagrams show that the generalised probability curves do not give very obviously better fits than the normal curve. The fact of the R. antero-lateral giving a strictly limited range while the L. antero-lateral gives an unlimited one, demonstrates that little stress can be placed upon the type of curve which a series of observations may yield.

In the present case the curves would appear to be sufficiently normal to allow us to find Galton's function (known as r) for pairs of organs. For this purpose we shall employ the modified formula

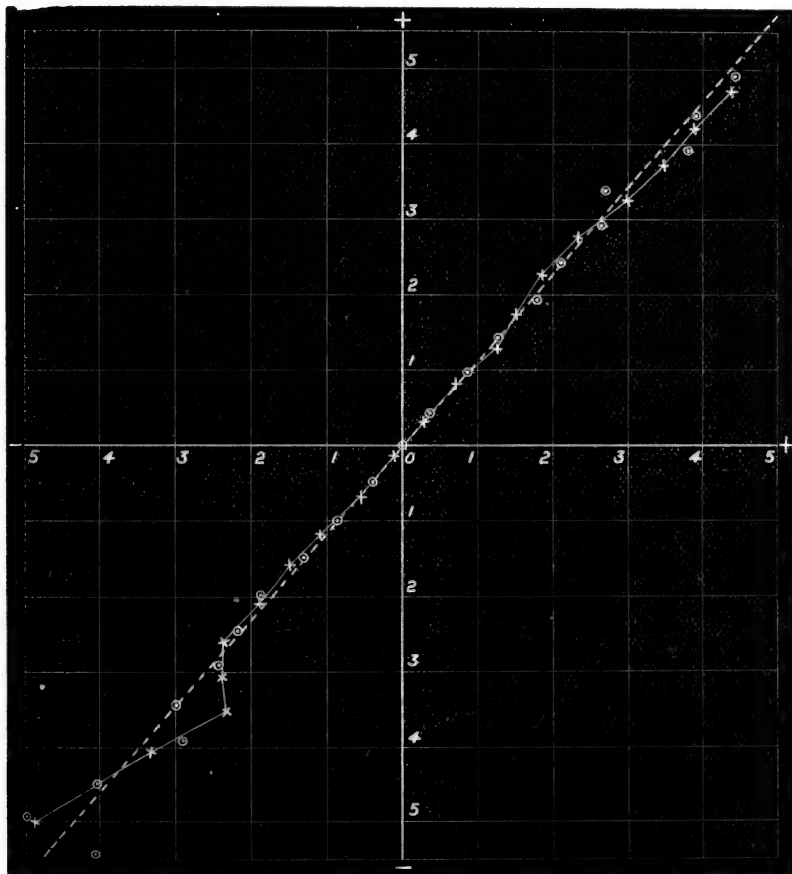
$$r = \frac{\Sigma \text{Deviation A} \times \text{Deviation B}}{n\sigma_A\sigma_B},$$

where n = number of individuals, and σ_A and σ_B are the standard deviations of organs A and B respectively. This formula has recently been shown by Professor Pearson to be superior to the one formerly used ('Phil. Trans.,' vol. 187).

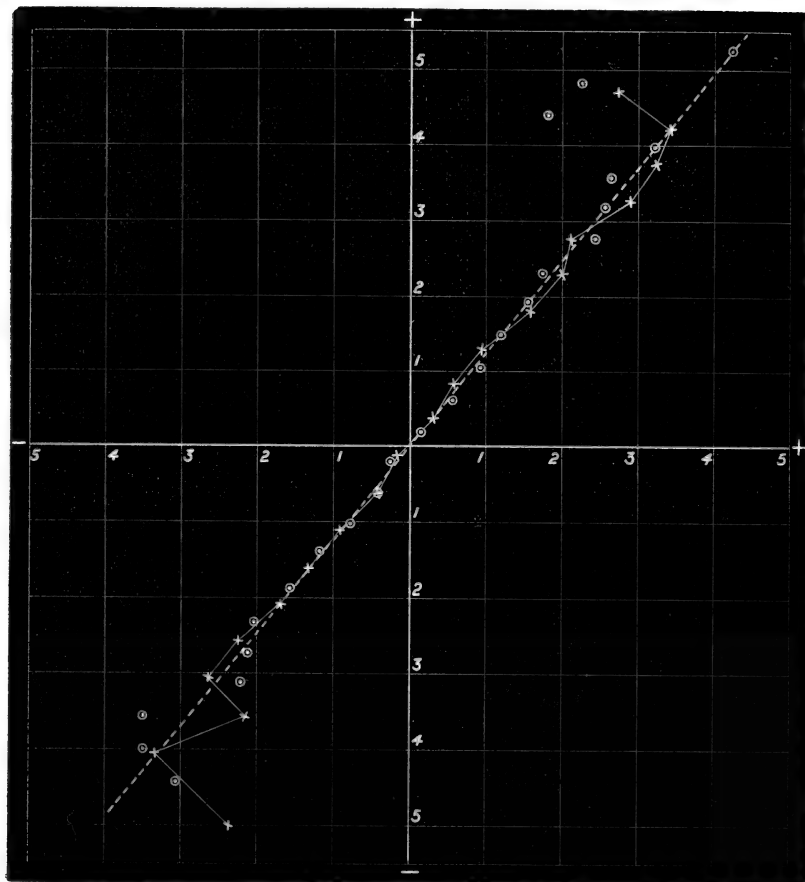
The correlation surfaces are published as being of permanent value, and it is believed that such material as this will be of use in the future in elaborating or modifying the current theory of correlation.

Figs. 5 and 6 represent two r lines. The crosses were obtained by

FIG. 5.—R. and L. Antero-lateral. $r = 0.86$.



Dotted line = the r line inclined to axis x at $\tan^{-1} 0.86$. The continuous line joining the crosses is the line found on taking R. antero-lat. as subject. The circles indicate the points obtained when the L. antero-lat. is the subject. The probable errors of the dimensions are represented by the intervals 1, 2, 3, &c., on both horizontal and vertical scales.

FIG. 6.—R. Antero-lateral and R. Dentary. $r = 0.80$.

Dotted line is the r line inclined to axis x at $\tan^{-1} 0.80$. The continuous line joining the crosses is the line found on taking the R. antero-lat. as subject. The circles indicate the points obtained when the R. dentary is the subject. The probable errors of the dimensions are represented by intervals, 1, 2, 3, &c., on both horizontal and vertical scales.

taking one dimension as the subject and the other as the relative, then for every unit of deviation of the subject the mean of the associated values of the relative was found. The values of the subject and the associated mean values of the relative are expressed in terms of their probable errors, and plotted along the axes of y and x respectively. The points indicated by circles were obtained by reversing the positions of subject and relative.

It is clear that the crosses and circles do tend to lie along a line

inclined at $\tan^{-1} r$ to the axis of x . The ends of the lines are irregular on account of the impossibility of obtaining a satisfactory mean to the relative at these points because of the paucity of individuals near the limits of the range of deviation. One sees that, in these two cases at least, the correlation surfaces must closely approximate to the symmetrical normal surface.

Organs.	<i>Carcinus moenas</i> . Naples race.		<i>Carcinus moenas</i> . Plymouth race.		<i>Portunus depurator</i> . Plymouth.	
	r .	Probable error of r .	r .	Probable error of r .	r .	Probable error of r .
Total breadth and frontal breadth.....	0·08	0·0211	0·10	0·0210	0·14	0·0305
Total breadth and R. antero-lat.....	0·66	0·0100	0·65	0·0103	0·67	0·0082
Total breadth and R. dentary.....	0·50	0·0143	0·55	0·0130	0·56	0·0107
Frontal breadth and R. antero-lat.....	0·29	0·0187	0·24	0·0195	0·30	0·0270
Frontal breadth and R. dentary.....	-0·23	0·0197	-0·18	0·0203	-0·03	0·0314
Frontal breadth and L. dentary.....	-0·26	0·0193	-0·20	0·0201	-0·01	0·0314
R. antero-lat. and L. antero-lat.....	0·76	0·0072	0·78	0·0066	0·86	0·0035
R. antero-lat. and R. dentary.....	0·71	0·0086	0·78	0·0066	0·80	0·0050
R. antero-lat. and L. dentary.....	0·60	0·0117	0·70	0·0089	0·74	0·0065

In the above table the 2nd and 4th columns give the values of Galton's functions which Professor Weldon found for two races of female *Carcinus moenas* ('Roy. Soc. Proc.,' vol. 54). The sixth column gives the values of r obtained for *Portunus*.

It is quite obvious that there is a marked similarity between the three columns of figures. The probable errors of r were found by the formula $0·6745 \frac{1-r^2}{\sqrt{n(1+r^2)}}$, which Professor Pearson shows will give a close approximation ('Phil. Trans.,' vol. 187). These probable errors were added to indicate how far the differences in the values of r are to be regarded as meaning actual deviations in the constants. The values obtained for the two races of *Carcinus* differ from one another nearly as much as they do from the constants of *Portunus*.

We have thus proved that the mutual relationships of the organs measured are almost as closely similar between the two genera *Portu-*

nus and *Carcinus* as between two not very sharply marked off races of a single species.

Of course a considerable number of such comparisons would be necessary before any safe conclusions could be drawn, and the meaning of the differences observed could only be discovered by such a comparative treatment of a large series of genera. It is probable that the larger deviations do indicate real differences in the correlation constant, possibly such are associated with changes in habit or environment. For example, it is conceivable that a crab which swims might require to be more symmetrical than one that only crawls between the tide-marks. *Portunus* does swim to a certain extent, and one can see from the table that the correlation of the two sides of the body is greater in this genus than in the essentially shore-living *Carcinus moenas*.

II. Correlation Surface of Total Breadth and R. Dentary.

1432 Individuals.

Measurements in thousandths of carapace length.		Total breadth.		R. dentary.	
1370—1367	17	10	436—439		
1366—1363	16	9	440—443		
1362—1359	15	8	444—447		
1358—1355	14	7	448—451		
1354—1351	13	6	452—455		
1350—1347	12	5	456—459		
1346—1343	11	4	460—463		
1342—1339	10	3	464—467		
1338—1335	9	2	468—471		
1334—1331	8	1	472—475		
1330—1327	7	0	476—479		
1326—1323	6	1	480—483		
1322—1319	5	2	484—487		
1318—1315	4	3	488—491		
1314—1311	3	4	492—495		
1310—1307	2	5	496—499		
1306—1303	1	6	500—503		
1302—1299	0	7	504—507		
1298—1295	1	8	508—511		
1294—1291	2	9	512—515		
1290—1287	3	10	516—519		
1286—1283	4	11	520—523		
1282—1279	5	12	524—527		
1278—1275	6	13	528—531		
1274—1271	7				
1270—1267	8				
1266—1263	9				
1262—1259	10				
1258—1255	11				
1254—1251	12				
1250—1247	13				
1246—1243	14				
1242—1239	15				
1238—1235	16				
1234—1231	17				
1230—1227	18				

Measurements in thousandths of carapace length.	Frontal breadth.	Total breadth.	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1	0	1	2	3	4	5	6	7	8	9	10	11	12
747-744	12	12																															
743-740	11	11																															
739-736	10	10																															
735-732	9	9																															
731-728	8	8																															
727-724	7	7																															
723-720	6	6																															
719-716	5	5																															
715-712	4	4																															
711-708	3	3																															
707-704	2	2																															
703-700	1	1																															
699-696	0	0																															
695-692	1	1																															
691-688	2	2																															
687-684	3	3																															
683-680	4	4																															
679-676	5	5																															
675-672	6	6																															
671-668	7	7																															
667-664	8	8																															
663-660	9	9																															
659-656	10	10																															
655-652	11	11																															
651-648	12	12																															

V. Correlation Surface of Frontal Breadth and R. Dentary.

460 Individuals.

Measurements in thousandths of carapace length.	Frontal breadth.	R. dentary.	448-451	452-455	456-459	460-463	464-467	468-471	472-475	476-479	480-483	484-487	488-491	492-495	496-499	500-503	504-507	508-511	512-515
747-744	12	7									1								
743-740	11	6										1							
739-736	10	5																	
735-732	9	4		1															
731-728	8	3					3		1	1	2	1			1				
727-724	7	2				2	1	1	2	1	3	1							
723-720	6	1			1		1	2	4	1	2	2			1				
719-716	5						1	5	4	1	1	1	2		2		1		
715-712	4				1		1	3	4	1	5	4	2		1		1		
711-708	3	1	1	1	4	3	3	4	6	2	5	5	3	6	1		2		
707-704	2	2	4	4	3	4	6	9	7	6	4	4	6	7	2	2	1		
703-700	1	1	6	2	1	6	7	7	7	7	3	7	6	7	2	2	1	2	1
699-696	0		3	3	3	6	3	6	8	5	8	4	3	4	3				0
695-692	1		1	3	3	4	4	2	5	8	4	7	3	4	1	2	1		1
691-688	2	1	1	3	1	2	4	2	4	4	5	3	4	3	1	1			2
687-684	3	1	1	3	1	1	2	3	4	4		2	2	1	1			1	3
683-680	4		1	1	1	2			1	1	2	3		2			1		4
679-676	5					1	3	2	4	2									5
675-672	6						2	1		1			2	2		1			6
671-668	7										1			1		1			7
667-664	8																		8
663-660	9															1			9
659-656	10																		10
655-652	11											1							11
651-648	12																		12
		7							0	1	2	3	4	5	6	7	8	9	

VI. Correlation Surface of Frontal Breadth and L. Dentary.
460 Individuals.

Measurements in thousandths of carapace length.	L. dentary.		Frontal breadth.	
747—744	12	9		12
743—740	11	8		11
739—736	10	7		10
735—732	9			9
731—728	8			8
727—724	7			7
723—720	6			6
719—716	5			5
715—712	4			4
711—708	3			3
707—704	2			2
703—700	1			1
699—696	0			0
695—692	1	1	1	1
691—688	2			2
687—684	3			3
683—680	4			4
679—676	5			5
675—672	6			6
671—668	7			7
667—664	8			8
663—660	9			9
659—656	10			10
655—652	11			11
651—648	12			12

1432 Individuals.

Measurements in thousandths of carapace length.	R. antero-lateral.	L. antero-lateral.
819—816	10	13
815—812	9	12
811—808	8	11
807—804	7	10
803—800	6	9
799—796	5	8
795—792	4	7
791—788	3	6
787—784	2	5
783—780	1	4
779—776	0	3
775—772	1	2
771—768	2	1
767—764	3	2
763—760	4	3
759—756	5	4
755—752	6	5
751—748	7	6
747—744	8	7
743—740	9	8
739—736	10	9
		10

“Investigations into the Segmental Representation of Movements in the Lumbar Region of the Mammalian Spinal Cord.” By WILLIAM PAGE MAY, M.D., B.Sc., M.R.C.P., Fellow of University College, London. Communicated by Professor VICTOR HORSLEY, F.R.S. Received July 1, 1896.

(From the Laboratory of the Physiological Institute, Berlin, and the Pathological Laboratory of University College, London.)

(Abstract.)

Introduction.

The following researches were carried out in consequence of suggestions made to me by Professor Victor Horsley, F.R.S., with the view of throwing light upon the degree to which certain movements, or, speaking more precisely, sensori-motor (kinæsthetic) phenomena are represented in any given segment of the lumbo-sacral region of the mammalian spinal cord, and further of determining what relationship exists between the representation of one movement and that of another. Of methods suggesting themselves for this investigation the one selected was direct excitation of the anterior or posterior roots or of the spinal cord itself.

Historical Introduction.

A series of laborious investigations has been carried out to determine the localisation of certain movements and the physiological relationship of various muscles in and to definite segments of the spinal cord, by Ferrier and Yeo, Bert and Marcacci, Forgue, Sherrington, and Russell, the method of which was limited (controlled by exclusion experiments) to stimulation of the motor roots. I am only aware of one antecedent localisation experiment (by Sherrington) carried out by stimulation of the posterior roots; nor can I find any record of the direct excitation of the surface of the cord for investigating the localisation of movement.

Method of Investigation and Precautions Observed.

(a) *Species of Animal.*—The animals chiefly employed were the dog and monkey (*Macacus sinicus* and *rhesus*).

(b) *Anæsthetic.*—The narcotic agents used were morphia and ether or, in the monkey, ether alone.

Operative Procedure.

Exposure of spinal cord.

Division of cord and isolation of segments.

The cord was exposed with due observation of well-known precautions (Gotch and Horsley, 'Phil. Trans.,' vol. 182, B, 1891). In some cases the spinal cord and roots were stimulated at first in continuity. In others, before proceeding to experiment, the spinal cord was completely divided at from two to eight segments above the part experimented upon. The spinal roots were divided as detailed in the paper.

METHOD OF EXCITATION.

I. *Electrical.*

Apparatus.—A single Daniell cell was used which supplied a Du Bois Reymond's inductorium of the usual type, the secondary coil being 20 cm. or more from the primary. The electrodes attached to the secondary coil consisted of closely approximated (1 mm.) platinum points. The duration of excitation was, as a rule, momentary, and never exceeded 1—2 seconds.

(a) *Excitation of Nerve Roots.*—The nerve roots were raised in the air and the electrodes usually applied, so that the direction of the exciting current was transverse to the nerve fibres.

(b) *Excitation of the Spinal Cord.*—The surface of the cord was gently dabbed with small wool swabs, kept in warm saline solution and squeezed dry, before the electrodes were applied. The duration of excitation was always brief, rarely exceeding one second.

The value of the method may be estimated by considering the following facts. On stimulation of the surface of the spinal cord as already mentioned, movement was always elicited in the leg on the side stimulated, when the electrode was applied to the surface of the posterior column, but never, as far as I was able to see, could movement be obtained by the application of this strength or even considerably greater strength of stimulus to the lateral or anterior columns, when adequate precautions (*vide* paper) were taken to prevent the direct spread of the current to the neighbouring root fibres. The movement elicited from stimulating the posterior columns was always marked and quite definite, and merely depended in intensity upon the conditions stated below. For instance, applying the electrodes to the surface of the postero-external column in the fifth lumbar segment of the dog on the left side produced lateral flexion of the spinal column to the same side, flexion and adduction of the hip, flexion of the knee and toes, and movement in the tail (flexion to the same side). But the chief result was the very local effect which could be obtained by varying the point stimulated; thus,

stimulation of a point 1 mm. centrally or laterally to a given point often produced an entirely different resulting movement or no movement at all, which fact is clearly of much importance in showing that, with the above strength of current, the restriction of the stimulus to one point can be accurately attained.

II. *Mechanical.*

As a means of controlling the observations derived from electrical excitation, mechanical stimulation was sometimes employed in examining the nerve roots, and was obtained by pinching the tissues with fine forceps. The results were precisely the same as those gained by electrical stimulation.

ON THE RESULTS OF DIRECT EXCITATION OF THE SURFACE OF THE SPINAL CORD IN THE DOG.

I. *Gross Localisation.*

(a) *Area Excitable.*—The excitable area of the surface of the cord itself is the postero-external column. Stimulation of the column of Goll produced no movement except in the lower lumbar region, where that column is either very narrow or practically absent, and where, presumably, the effect was due to the stimulus directly affecting the fibres of the postero-external column.

(b) *Unilaterality.*—In the large majority (91·5 per cent.) of experiments on animals (dog, cat, monkey) the fact was strikingly evident that the movements produced were limited to the side stimulated.

(c) *Vertical Extent of the Spinal Cord in the Dog from which movement in the Lower Limb can be obtained.*—In the dog, movements in the lower limb can be produced from stimulation of Burdach's column from the upper border of the 13th dorsal segment to the lower border of the 1st sacral segment, and from the results obtained it will be seen that the various areas in the postero-external column, the stimulation of which on the surface of the cord produces movements in the limbs, anus, and tail, all overlap one another, but that on the whole the hip area is a little nearer the cerebrum than that for the knee, the area for the knee more proximal than that for the foot, and so on.

(d) *Effect of Transversely Dividing the Cord above the Lumbar Enlargement.*—The only effect observed to follow such separation of the cord from the brain, upon the movements elicited as above described, was one of increased excitability. The limits described were found to prevail precisely, and the cord was excitable quite up to the level of the section.

II. Minute Localisation within the Excitable Area.

Repeatedly it was found that with a minimal stimulus it was possible to evoke movement either in the tail (and anus) only, or in the hamstrings, or in the hip or in the side only, and whenever this was obtained it was an invariable rule that the point for producing movement in the tail was placed in the cord mesially of that point, stimulation of which gave movement in the hamstrings, and that this latter point was mesial of that for the hip, while most external of all was the point from which movement of the side of the trunk was elicited. This lateral arrangement has been in part foreshadowed by the observations of Mott on the relation between the coccygeal nerves and Goll's column.

Investigation into the Segmental Representation of the Cord by Comparison of the Results of Excitation of the Anterior and Posterior Roots.

(a) *Latency of Effect.*—Of course, in accordance with all previous investigations, the delay in passing through the spinal cord was well marked.

(b) *Character of Movement Elicited from the Respective Roots.*—Stimulation of the peripheral end of an anterior root gave, on the whole, a quick powerful *extension* of the whole limb, the latency, of course, being extremely short; on the other hand, excitation of the corresponding posterior root resulted in a slower, though strong, *flexion* of the whole limb with a well-marked latency.

This remarkable functional distinction between the roots, viz., anterior giving *extension* and posterior *flexion*, was quite constant, and was obtained in every animal in which the experiment was made. Of course, the movement which took place was a resultant effect, and was produced by the contraction of many muscles, each muscle contracting in whole, or in part, in combination with other muscles to produce the extension or flexion respectively.

The results with each root are given in the tables.

Further, stimulation of a posterior root (say the 5th) produced flexion of a joint or joints even when all the neighbouring anterior roots but one were divided. Hence this flexion can only be due to the stimulus passing from the posterior root through the spinal cord along a particular anterior root to the muscles (differentiation of function in the nerve centre of that root), yet stimulation of this same anterior root produces extension.

And this agrees entirely with the results obtained in a different way by Dr. Risien Russell ('Phil. Trans.,' 1893).

The above experiment also goes to show that stimulation of one posterior root causes impulses to pass out along many anterior roots.

A further important condition of the particular function with which we are now concerned (sensori-motor reflex) is that, from the present investigation, it seems certain that the path along which the impulses pass as evidenced by movement elicited in stimulating a certain posterior root, is directed towards a point *below* the level of that posterior root, and, not as we might suppose, chiefly in the same segment, or even above the level at which the posterior root joins the cord.

The proof of this new conclusion is afforded by many facts given in the paper, not the least interesting of which is, that on direct stimulation of the second or third anterior lumbar roots in the dog, no movement results in the lower limb, yet stimulation of the third posterior lumbar root gives distinct flexion and adduction of the hip and flexion of the knee, and stimulation of the second posterior lumbar root gives slight flexion of the hip and knee. In this connexion also results obtained by Claude Bernard, Schültze, Ramon-y-Cajal, Kölliker, Retzius, and Golgi afford similar evidence.

Influence of the Posterior Roots upon the Nerve Centres in the Spinal Cord.

It was found that repeated excitation of the posterior roots decidedly increased the excitability of the posterior roots themselves, of the spinal cord and of the anterior roots. The difference in the excitability of the preparation before and after the previous stimulation may be represented by the fact that, whereas the minimal stimulus before the application of the repeated stimulus was represented by a distance of 50 cm. of the secondary from the primary coil in the condition of heightened excitability, a minimal stimulus was obtained at a distance of 70 cm. On the other hand, by cooling the posterior roots as suggested by Professor Gad, a converse effect was produced. The results of Belmonda and Oddi are also quoted in this connexion.

Results of Experiments upon the Spinal Cord in the Monkey.

Method.—The same as above.

The same general results were obtained by stimulation of the spinal cord in the monkey as described in the case of the dog. The area found excitable was the postero-external column, stimulation of which from the thirteenth dorsal segment to the second sacral segment inclusive produced after a very short latent period lateral flexion of the spinal column, flexion and adduction of the hip, flexion of the knee, ankle, and toes, movement of the tail and perineum, and peristalsis (rumbling of the bowels), though it depended on the position of the electrode and the strength

of the stimulus whether only a part or the whole of these movements resulted. Similarly, with regard to the lateral extent of the areas mentioned above, although, of course, a strong stimulus caused movement in all parts named, yet in each case it was quite definite that the area, stimulation of which produced movement in the tail, was nearer the middle line than that for the hamstrings, the area for the hamstrings nearer the middle line than that for flexion of the hip, and this median of that which caused lateral flexion of the side. Hence these results demonstrate from a functional standpoint the anatomical arrangement which has been described by various writers—Ramon-y-Cajal, Kölliker, Golgi, &c.

As in the dog, the knee jerks were found not only present, but even exaggerated, after the cord had been completely divided.

Also section of the cord caused increased excitability of the parts below section, but abolished the movement produced by stimulation of the first or second or third posterior roots below the level of the section, and the more oblique the position of the posterior root fibres in contact with the cord, the greater the interval affected in this manner.

On the Production of Movement by Stimulation of the Anterior and Posterior Spinal Roots in the Monkey.

The general results obtained by excitation of the spinal roots in the monkey were the same as those in the dog. As already known (Sherrington, Risien Russell) stimulation of the third lumbar to the first or second sacral anterior roots (inclusive) alone produces movement in the lower limb, yet, on stimulation of the posterior roots of the twelfth dorsal to the second sacral inclusive, it was found that movement resulted in the lower limb, and in the latter case the bulk of the movement produced is that of flexion.

Summary and Conclusion.

1. *Relationship of Posterior Roots to Reflex Kinæsthetic Centres.*—It appears from the foregoing experiments to be definitely established that any reflex centre derives its chief afferent impulses from a nerve root which enters the cord, as a rule, about two segments higher, *i.e.*, on the cephalic side. This generalisation, established by the method of excitation, is confirmed by anatomical and pathological considerations.

2. *Lateral Arrangement of Fibres in Burdach's Column.*—The fibres of the postero-external column are arranged in a definite and constant order from within out, the innermost fibres (*i.e.*, those nearest the middle line) representing the most distal portions of the tail and lower limb and the outermost the proximal segments of the limbs.

3. Whereas direct excitation of the anterior roots in the dog produces, as a resultant movement, extension of the lower limb, the resultant movement produced from the kinæsthetic centres of excitation of the posterior roots is always flexion. In the monkey there is not this apparent antagonism, because stimulation of the anterior roots in that animal brings out a differentiation of flexion and extension, although excitation of the posterior root gives flexion alone.

“Preliminary Statement on the Development of Sporangia upon Fern Prothalli.” By WILLIAM H. LANG, M.B., B.Sc., Lecturer in Botany, Queen Margaret College, and Robert Donaldson Scholar, Glasgow University. Communicated by D. H. SCOTT, M.A., Ph.D., F.R.S., Honorary Keeper of the Jodrell Laboratory, Royal Gardens, Kew. Received September 14, 1896.

The observations recorded in this paper were made in the course of an investigation into the relation existing between variability in the fern plant and apogamy in the prothallus. This research was undertaken at the suggestion of Professor Bower, F.R.S., and has hitherto been conducted in the Jodrell Laboratory, Royal Gardens, Kew. To Dr. Bower and Dr. Scott I am indebted for valuable assistance and advice.

In two of the species investigated, *Scolopendrium vulgare*, L., and *Lastræa dilatata*, Presl., sporangia were borne upon the prothallus. In the former they were sometimes associated with apogamous development of the sporophyte, the details of which differ, however, from previously recorded cases of apogamy. As a considerable period must elapse before an amount of material sufficient for the complete study of details of development can be obtained, it appeared advisable to describe the results obtained from the material at present available. Cultures are about to be commenced in the Glasgow Botanic Gardens for the further study of these abnormal prothalli.

The prothalli of the two species investigated will first be described, and the theoretical bearing of the results briefly considered.

Lastræa dilatata, Presl., var. *cristata gracilis*, Roberts.

The spores from which the cultures of this fern were made were obtained from a plant in the collection of Mr. C. T. Druery, F.L.S., who kindly supplied me with material. This variety was found wild in Carnarvon in 1870. Spores were sown in the first week of November, 1895, upon a carefully sterilised soil, consisting of

a mixture of vegetable mould and sand. The pot was kept constantly covered with a glass plate, and the necessity of watering was avoided by standing the pot in a large saucer kept full of water. A close crop of well-formed prothalli, on which antheridia and archegonia were present, completely covered the surface of the soil. In April, 1896, a number of the prothalli bore normal embryos in an early stage of development. Three months later numerous young plants were present, which were found on examination to be normally produced.

The prothalli which had not been fertilised had lost the heart-shaped outline and elongated considerably; some of them reached a length of 2 cm., and were 5 mm. in breadth. The archegonia were very numerous, and were situated upon a distinct cushion, which was continued in the larger prothalli as a well-marked midrib. They were arranged in transverse rows; their necks had opened in a normal manner, and the canal showed the usual brown discoloration. Antheridia were present on some of the prothalli.

In some of these prothalli the midrib was continued into a cylindrical process of variable thickness. This arose in some examples as a direct continuation of the apex, but more frequently was attached to the under surface, just behind the apex of the prothallus; in one instance it was found in a corresponding position on the upper surface. The actual apex usually loses its meristematic appearance; it grows out as a narrow triangular lobe, which consists of colourless cells, and contains tracheides. This lobe closely resembles the "middle lobe"* found in the apogamous prothalli of certain ferns, and probably corresponds to it. In a few instances this middle lobe is formed, but no cylindrical process arises; in such cases secondary prothalli are produced from the anterior margin of the thin lateral wings, and the whole closely resembles an aborting prothallus of *Aspidium filix-mas* or *Pteris cretica*. When the prothalli are seen from above, the anterior edge can be traced across the base of the cylindrical process. As will be described below, the first sporangia formed on the prothallus are usually situated on this margin, especially on the "middle lobe." The process is of the same deep green colour as the midrib. Sexual organs, often in considerable numbers, are borne upon it. They are usually well formed; the archegonia open in the usual manner, and the spermatozoids are capable of active movement when liberated. On other examples variously malformed sexual organs occur. The abnormal archegonia are seated upon small elevations composed of cells which contain chlorophyll; sometimes the neck is open, but other examples have the

* Farlow, 'Quart. Journ. Microscop. Sc.,' 1874, p. 268. De Bary, 'Bot. Zeit.,' 1878, p. 463.

neck closed and branched. The central cell of the abnormal antheridia is arrested at a more or less early stage of development, while the cells of the wall and the base take on active growth.

The sporangia are either isolated or associated together in groups, which bear a striking resemblance to sori. They are borne upon the process or close behind it upon the true middle lobe, and are rarely found upon prothalli which have not produced a cylindrical process. When this is the case, they are always isolated and situated on the edge of a thin continuation of the prothallus arising from the apical depression.

Single sporangia occur frequently on the edge of the prothallus, which, as described above, crosses the base of the process. In a number of examples a single sporangium occupied a median position, and, from earlier stages observed, it is probable that it is to be traced back to the original growing point of the prothallus. In other cases several sporangia were formed in this region. Isolated sporangia are also found on the process, but more frequently groups are met with. They occupy the upper or lateral faces of the process, and whenever sporangia in early stages of development are found, they are situated on its apex. It is probable that the groups of older sporangia had become displaced from this position by the further growth of the process. The groups were at a considerable distance from each other.

The relative positions of sporangia and sexual organs is a point of some interest, and was readily determined. Archegonia were present close to the sporangia, and at the same level on the process. When the process, after producing sporangia, had continued its growth, archegonia and antheridia were present on the portion beyond the sporangia, as well as on the older part, and, in cases in which more than one group of sporangia had developed, the intervening region bore sexual organs. Rhizoids are also produced abundantly from the shaded side of the process, and, so far as external appearance is concerned, there is no reason to doubt the prothallial nature of the region on which the sporangia are situated. The tissue underlying the sporangia, however, presents peculiarities in structure which may modify this conclusion to some extent. Beneath the single sporangia developed on the edge of the prothallus a few tracheides, which agree in every respect with those present in apogamous prothalli, were always to be found. Similar elements were always present in the tissue beneath the groups situated on the process. It is possible that here, as in the case of the sporangia upon the prothallus edge, the first tracheides are developed before the young sporangium can be recognised. All that can be stated with certainty is that they are already present beneath very young sporangia. The tracheides may become connected together into a

band, resembling a rudimentary vascular bundle, and suggesting a comparison with the vascular supply of a sorus.

The development of the sporangium could not be followed in detail in the material obtained as yet, but a sufficient number of stages have been found to make it clear that there is no difference of importance from the well known course of development of the same member on the sporophyte. In the youngest stage seen the apex of the sporangium was occupied by a tetrahedral cell, the cells destined to form the lateral portions of the wall having already been cut off from a large, dome-shaped terminal cell, the limits of which were clearly recognisable. This was borne upon a stalk cell. A tetrahedral archesporium is formed, from which tapetal cells are cut off. The tapetum subsequently becomes two-layered, and the central cell develops into a group of sporogenous cells. From these, in the most mature sporangia found, a number of dark brown spores had developed, while the tapetum was represented by numerous granules between the spores. The number of spores appeared to be the same as was contained in a sporangium developed on the sporophyte. The sporangium wall was perfectly developed; the cells of the annulus showed the characteristic thickening of their walls, which were of a dark brown colour, and a well formed stomium was present. When tested with dehydrating agents, the mechanism of the annulus was found to be perfect. The stalk consisted of four rows of cells.

No sporangia have been found in which the spores were ripe, but in view of the advanced stage of development in those observed, there is every probability that some may be obtained. It will be interesting to ascertain if the spores are capable of germination, and if the prothalli produced show any peculiarities. The spores seen already possessed a thick wall on which indications of sculpturing were apparent, and a single nucleus was present in each.

When the unnatural conditions under which they developed are borne in mind, it is not surprising that many imperfect sporangia were found. Such sporangia were in fact the more numerous. Sometimes the arrest of development had taken place before the tapetum had originated from the archesporium, but more commonly the double layer of tapetal cells was present surrounding a sporogenous cell which had become highly refractive, the nucleus being indistinguishable. The annulus could be made out, but its cells were thin walled and colourless, and the whole sporangium was pale and more flattened than one of the same age in which sporogenous tissue had formed.

No evidence has yet been obtained of the production of sporophytes, showing vegetative organs upon the cylindrical process, but one example was seen in which a group of sporangia, situated on the apex of the process, was surrounded by ramenta.

Scolopendrium vulgare, L., var. *ramulosissimum*, Woll.—The cultures of this fern were made in the manner already described for *Lastræa dilatata*. The spores were obtained from a plant grown in the open air in the Royal Gardens, Kew.

The prothalli were at first heart-shaped, and on many of them normally produced embryos developed. No further changes ensued in those on which young plants were present, and they soon became colourless and died. In those which had remained unfertilised, however, the apex continued directly into a cylindrical process,* which was of considerable thickness, and in some cases attained a length of 5 mm. The lateral portions of the prothallus showed no further growth, and became in time brown or colourless appendages to the base of the cylindrical process. On the process were numerous archegonia, and its prothallial nature was still further shown by the presence, in some instances, of thin lobes of tissue, which generally bore antheridia. Sections through the process in this stage show that the archegonia are normally formed, and reach almost to the apex, and that tracheides are absent from the tissue. The archegonia are capable of fertilisation, for in some instances normally produced embryos were found.

After the process has in this manner attained a greater or less length, its tip becomes yellowish, contrasting with the deep green colour of the region behind. Near the apex ramenta develop, which soon completely clothe the tip of the process and render it white and conspicuous. Archegonia are present to just below the ramenta. Longitudinal sections at this stage show that one or two small elevations corresponding to the rudiments of the apex of the stem, and the first leaf of the sporophyte have been formed. Beneath the broad tip a flat mass of small meristematic cells extends; the meristematic tissue is continuous with that of the stem and leaf apices, but, on passing away from these, is separated by several layers of large, non-meristematic cells from the surface. In a slightly older stage the stem apex has become conical, and a number of leaves have formed which are circinately curved, and form a bud clothed with ramenta. In the meristematic mass numerous tracheides have been developed. One large group is central in position, and extends to the limit between prothallial and sporophytic tissue, while others are found beneath the bases of the leaves, and are in continuity with their procambial strands. The apex of the stem is occupied by an initial cell, the relation of which to the initial cell or cells of the apex of the process has not yet been traced. The young sporophyte appears to be a direct continuation of the process. It is possible that some of

* Prothalli of *Scolopendrium*, which from the brief description given of them appear to have borne similar processes, are mentioned by E. J. Lowe, in the 'Gard. Chron.,' November 10, 1895. They were not investigated further.

the cases of apogamy recorded by Stange* were of this nature, but in *Doodia caudata*, R. Br., which is the only one of his species yet investigated in detail,† the elevations, from which sporophytes developed, were situated on the under surface of the prothallus. This case appears to be intermediate in character between *Scolopendrium* and the species investigated by De Bary.‡

Several prothalli were found bearing sporangia; these were grouped together in large numbers, usually upon the upper surface of the cylindrical process, but sometimes both above and below. Archegonia were situated close to the groups of sporangia. In the region of the prothallus, underlying the group, a strand of tracheides was found; in one instance this was connected with a spherical mass of tracheides developed to all appearance within the venter of an archegonium whose neck had not opened. The tissue upon which the sporangia are inserted is thin walled, and its cells have granular contents; it contrasts sharply with the cells of the prothallus which have a large vacuole and walls which stain much more deeply with hæmatoxylin.

As in the case of *Lastræa dilatata*, the stages seen render it probable that the sporangia follow the usual course of development. Two layers of tapetal cells are formed which surround a considerable mass of sporogenous tissue. Many of the sporangia fail to attain full development; they remain colourless, and in time wither. A few have been found, however, with a well developed annulus of a dark colour; these contained spores which have not, however, been examined in detail.

In one case two ramenta overarching a group of sporangia were seen. At first sight it seemed possible that they might correspond to an indusium, but, when taken in connexion with another example in which a cylindrical process, which bore sporangia laterally, terminated in an apogamously produced bud, another explanation appears more probable; this will be referred to again below.

It is worthy of note that another variety of this species has been found to produce young plants, the first fronds of which bore numerous prothalli while still in connexion with the stem.§ The prothalli on which these plants appeared had been subjected to repeated subdivision, a process which in other species|| has been found to induce apogamous development of the sporophyte. Unfortunately nothing is known of the manner in which these peculiar plants of *Scolopendrium* were produced, but it is possible that they arose apogamously. The case of *Scolopendrium* would then be com-

* 'Ber. der Gesellsch. f. Bot.,' Hamburg, 1886, p. 43.

† Heim, 'Flora,' 1896, p. 329.

‡ *Loc. cit.*

§ In a paper by Mr. E. J. Lowe, read at the Linnean Society, February 20, 1896.

|| Stange, *loc. cit.*

parable to that of *Trichomanes alatum*,* in which apogamy and apospory co-exist. Prothalli have been found to arise directly from the older fronds of another variety of *Scolopendrium*.†

An attempt will now be made to bring the peculiar modification of the life-history cycle of these ferns into relation with previously recorded cases of apogamy, and to estimate its theoretical bearing. A full consideration of these points must be deferred until more extended observations have been made.

There seems no reason to doubt the prothallial nature of the cylindrical process: its origin, the character of its cells, the presence of functional sexual organs, the development of rhizoids, and the direct transition to an ordinary flat prothallus apex sometimes met with, are sufficient grounds for this conclusion. The distinction between its origin as a direct continuation of the prothallus, and the cases in which it arises behind the apex which has lost its meristematic character, is not an essential one. Both forms occur in *Lastræa dilatata*; in the latter case the process may be compared with the numerous elevations which appear on the under side of old prothalli of *Doodia caudata*,‡ and are capable of apogamous development. The formation of such processes by prothalli which have attained a considerable size without having been fertilised, appears to be of not infrequent occurrence, and is usually associated with apogamy. It is recorded in *Todea pellucida*, Carm., *T. rivularis*, Sieb.,§ and *Athyrium filix-fœmina*, Bernh.,|| and the writer has found in *Aspidium frondosum*, Lowe, as many as six apogamous buds, formed from the tips of cylindrical processes, which arose from the anterior margin of a prothallus.

The term cylindrical process¶ has been used to avoid confusion with the middle lobe developed in aborting prothalli of *Pteris cretica* and *Aspidium filix-mas*. This, as De Bary has shown, may be regarded as corresponding to some extent with the first leaf of an apogamous sporophyte.** A structure comparable with this middle lobe has been found in prothalli of *Lastræa dilatata*, which had also produced a cylindrical process; usually one or more sporangia were borne upon it.

Tracheides were always present in the tissue beneath sporangia,

* Bower, 'Annals of Botany,' vol. 1, p. 300.

† Druery, 'Linn. Soc. Journ.,' vol. 30, p. 281.

‡ Heim, *loc. cit.*, p. 340, fig. 12.

§ Stange, *loc. cit.*

|| Druery, 'Gard. Chron.,' November 10, 1895.

¶ It is impossible to determine whether the structure to which Wigand ('Bot. Zeit.,' 1849, p. 106) applied this name, and which he inclined to consider as a rudimentary axis, was of the same nature or was a true middle lobe, but the latter appears the more probable conclusion.

** *Loc. cit.*, p. 464.

and the question arises whether their occurrence is to be regarded as of morphological significance. They have been found in the prothalli of a number of species of ferns, and, in every case investigated, were associated with apogamy. In the case of *Pteris cretica* the differentiation of the tracheides in the prothallus precedes the origin of the bud.* This is the case also with the single sporangia formed on the edge of the prothallus, and probably holds good for the groups of sporangia borne on the process. But tracheides may occur in the prothallus at a distance from the place of origin of buds or sporangia. Putting aside the case of the middle lobe, the prothallial nature of which is open to doubt, a large bundle of tracheides was found in the substance of a fleshy prothallus of a variety of *Scolopendrium vulgare*, which bore numerous archegonia on the surfaces immediately above and below the tracheides. Elongated cells, which resemble sclerenchyma fibres, occur in the midrib of certain frondose liverworts.† A still more instructive example is afforded by the presence of tracheides in the massive endosperm of certain cycads.‡ This latter case shows clearly that such elements may be formed in the gametophyte to meet a physiological need. It seems inadvisable, therefore, to lay stress on the presence of tracheides as a means of distinguishing between the two generations, and the more so since their occurrence in a portion of the prothallus which is about to bear a bud or sporangia can be recognised as a physiological advantage. Such means of procuring a sufficient water supply may be a necessary preliminary to the development of a young sporophyte or a group of sporangia.

Lastly, it remains to consider the view to be taken of the presence of the characteristic reproductive organs of the asexual generation upon the gametophyte, and to consider its bearing upon the nature of alternation of generations in the archegoniatae. Since the discovery that in certain cases the one generation could arise directly from the other without the intervention of the proper reproductive organs, such cases have been used in support of the view that the alternation in the Archegoniatae was homologous.§ On the other hand, it has been maintained, both on grounds of the exceptional nature of these cases of apospory and apogamy, and of comparative phylogeny, that the distinction between the two generations was a much deeper one; that the alternation was not homologous, but antithetic.|| So far no case has been recorded in which the proper reproductive organs of the one generation were situated upon the

* Farlow, *loc. cit.*, p. 269.

† Goebel, 'Outlines,' p. 145.

‡ I am indebted to Professor Bower for this unpublished fact.

§ Pringsheim, 'Jahrb. f. Bot.,' bd. 9, p. 43.

|| Bower, 'Annals of Botany,' vol. 4, p. 347.

other without the intervention of the vegetative organs. At first sight such appears to be the case in the prothalli of the two species described; sporangia were present in close proximity to the sexual organs, the vegetative organs of the sporophyte being, at most, represented by a mass of cells underlying the group of sporangia, and even this distinction may not be recognisable beneath the single sporangia on the edge of the prothallus.

Several reasons may be adduced, however, against regarding these phenomena as evidence that the alternation of generations found in the ferns is not antithetic. In the first place, it is to be noted that the two forms in which sporangia have been observed upon the gametophyte are highly variable species, and that the varieties studied were well-marked crested forms. Further, the conditions under which the prothalli existed were in several respects unnatural. Among them the fact that fertilisation was prevented by not watering the cultures from above, and that a prolonged growth of the unfertilised prothalli was thereby induced, is of special interest, for it appears that apogamy is liable to occur under such conditions in ferns which, as a rule, reproduce sexually. While these considerations do not of themselves preclude deductions being made from these peculiar forms of reproduction, they necessitate especial caution in their use in the discussion of broad morphological questions.

Further, a number of reasons exist for considering the production of sporangia on the prothallus as a special case of apogamy. In *Scolopendrium vulgare* a sporophyte may develop from the tip of the cylindrical process. This may happen after a group of sporangia has been developed. In one case two rammenta were present, one on each side of a group of sporangia; they were in every respect similar to the rammenta which develop on the tip of the process when it is being transformed into the apex of a bud. Whenever a group of very young sporangia was seen it was situated upon the apex of the lobe, and the sporangia were in a more advanced stage of development the farther the group to which they belonged was removed from the apex. This has been most clearly seen in the case of *Lastrea dilatata* in which no buds with vegetative organs have as yet been seen, although in one case rammenta were associated with the sporangia, but it also holds for *Scolopendrium*. The explanation of these facts, which appears most probable, is that each group of sporangia had occupied the apex of the process when very young, and had become farther removed from this position as the process continued to increase in length. It is uncertain whether this growth is by direct continuation of the original growing point of the process, or whether the development of a group of sporangia at the apex necessitates the formation of a new growing point; possibly both forms occur. If the latter be the case a process on which several

groups of sporangia are present must be looked upon as a sympodium. Some probability is lent to this view by the fact that the first appearance of the process in *Lastræa* is usually as a sympodial continuation of the axis of a prothallus whose true apex has developed one or more sporangia.

Since the group of sporangia and the tissue of peculiar character on which they are seated are developed in the place of an apogamously produced vegetative bud, they may be looked upon as constituting a very reduced sporophyte. The drain upon the resources of the prothallus entailed by the production of this reduced bud, which is incapable of further growth, is much less than when a vegetative bud is formed. This explains why a number of such sporangial groups can be produced and supported by a single prothallus. The occurrence of a number of vegetative buds on a single prothallus is the exception, but may happen, as the case of *Aspidium frondosum*, before mentioned, shows.

It is probable that it is in the constitution of the nuclei that a means of distinction between cells of the oophyte and the sporophyte must be looked for in these cases in which the two generations are in intimate connection with each other.*

The complete life history of the fern is in these cases still further shortened than in the ordinary cases of apogamy; not merely the formation of a zygote by the fusion of antherozoid and ovum, but the formation of an embryo, in which any differentiation of the vegetative organs can be detected, is omitted, and the sporophyte is reduced to a mass of tissue which may be compared to a placenta bearing sporangia. The occurrence of single sporangia upon the edge of the prothallus may, in the light of the series of stages described, be considered as a still further case of reduction of an apogamous sporophyte. While this does not altogether prevent the explanation of the presence of sporangia upon the prothallus from the point of view of the supporters of the homologous nature of the two generations, it brings the present case into line with other exceptions to the normal life-history cycle, whose bearing on the nature of alternation has been discussed by Bower.† The present case, although more striking in its appearance, seems, so far as it has been investigated, to afford no sufficient reason for dissenting from the conclusion at which he arrived.

It is of interest to note the additional evidence, were such needed, which these observations afford of the generalization made by Goebel,‡ that the sporangium is to be regarded as an organ *sui generis*.

* Bower, 'Trans. Bot. Soc. Edinb.,' vol. 20.

† 'Annals of Botany,' vol. 4, 1890, p. 347.

‡ 'Bot. Zeit.,' 1881, p. 707.

From the staff of the Royal Gardens, Kew, I received ready assistance in many practical matters in the conduct of the cultures; my thanks are especially due to the curators, Mr. Watson and Mr. Nicholson.

November 19, 1896.

Sir JOSEPH LISTER, Bart., President, in the Chair.

Dr. Francis Elgar was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Mr. Shelford Bidwell, Professor Bonney, and Mr. Horace Brown were by ballot elected Auditors of the Treasurer's accounts on the part of the Society.

The Secretary read the Titles of the Papers received since the last meeting, which, under the new Standing Orders, had been published (see 'Proceedings,' No. 362).

The following Papers were read:—

- I. "The Reproduction and Metamorphosis of the Common Eel (*Anguilla vulgaris*). By G. B. GRASSI, Professor in Rome. Communicated by Professor E. RAY LANKESTER, F.R.S.
- II. "Total Eclipse of the Sun, 1896.—The Novaya Zemlya Observations." By Sir GEORGE BADEN-POWELL, K.C.M.G., M.P. Communicated by J. NORMAN LOCKYER, C.B., F.R.S.
- III. "Preliminary Report on the Results obtained with the Prismatic Camera during the Eclipse of 1896." By J. NORMAN LOCKYER, C.B., F.R.S.

"The Reproduction and Metamorphosis of the Common Eel (*Anguilla vulgaris*). By G. B. GRASSI, Professor in Rome. Communicated by Professor E. RAY LANKESTER, F.R.S. Received October 19, 1896. Read November 19, 1896.

Four years of continual researches made by me in collaboration with my pupil, Dr. Calandruccio, have been crowned at last by a success beyond my expectations, that is to say, have enabled me to

dispel in the most important points the great mystery which has hitherto surrounded the reproduction and the development of the Common Eel (*Anguilla vulgaris*). When I reflect that this mystery has occupied the attention of naturalists since the days of Aristotle, it seems to me that a short extract of my work is perhaps not unworthy to be presented to the Royal Society of London, leaving aside, however, for the present, the morphological part of my results.

The most salient fact discovered by me is that a fish, which hitherto was known as *Leptocephalus brevirostris*, is the larva of the *Anguilla vulgaris*.

Before giving the proofs of this conclusion I must premise that the other Murænoids undergo a similar metamorphosis. Thus, I have been able to prove that the *Leptocephalus stenops* (Bellotti), for the greatest part, and also the *Leptocephalus morrisii* and *punctatus* belong to the cycle of evolution of *Conger vulgaris*; that the *Leptocephalus hæckeli*, *yarrelli*, *bibroni*, *gegenbauri*, *köllickeri*, and many others imperfectly described by Facciola, and a part of the above-named *Leptocephalus stenops* of Bellotti, belong to the cycle of evolution of *Congromuræna mystax*; that the *Leptocephalus tænia*, *inornatus*, and *diaphanus* belong to that of *Congromuræna balearica*; that under the name of *Leptocephalus kefersteini* are confounded the larvæ of various species of the genus *Ophichthys*; that the *Leptocephalus longirostris* and the *Hyoprurus messanensis* are the larvæ of *Nettastoma melanurum*, and that the *Leptocephalus oxyrhynchus* and other new forms are larvæ of *Saurenhelys cancrivora*, and that finally a new little *Leptocephalus* is the larva of *Muræna helena*.

The form known as *Tylurus* belongs to *Oxystoma*, of which we unfortunately know nothing more than a figure by Raffinesque. I have not been able to find the *Leptocephalus* of *Myrus vulgaris*, of which I have had only a single young individual, in which the transformation was already far advanced. Neither have I found the *Leptocephalus* of *Chlopsis bicolor*, a very rare form, which is related to *Muræna* and to *Murænichthys*. As the result of these observations, the family of the *Leptocephalidæ* has been definitely suppressed by me; the various forms of that family are, in fact, the normal larvæ of the various Murænoids.

In regard to the greater part of the above-named species, the control has been threefold, namely:—

Firstly, anatomical. I have compared the various stages in all their structures, and have made the due allowance for the changes brought about by the metamorphosis at the close of larval life.

Secondly, natural. I have found in nature all the required transitional stages.

Thirdly, experimental. I have followed, step by step, the metamorphosis in aquariums.

Therefore, the hypothesis of Günther that the Leptocephali are abnormal larvæ, incapable of further development, must be rejected. All this is related by myself at length, with all historical details which concern the question, in a large memoir which is about to appear in the Journal edited by Professor Todaro.

Until now all these facts have been unknown because normally they can only be observed in the abysses of the sea at a depth of at least 500 metres. Fortunately, along a part of the coast of Sicily strong currents occur, which must be ascribed to the tide, producing very large displacements of the water in the narrow Strait of Messina. I shall give further details concerning these currents in my large memoir. In consequence of the strong currents, sometimes—I say sometimes, because there is no regularity, and one may have to wait for a year without obtaining any material—not only many deep-sea fishes, but also all stages of the development of the Murænoids are met with in the surface-water. To these currents we owe all the captures of *Muræna helena* with ripe eggs, which is in accordance with what I had already argued from other facts, namely, that the reproduction of the Murænoids takes place at great depths of the sea.

Before I proceed to speak of the Common Eel, I must premise that Dr. Raffaele has described certain pelagic eggs as belonging to an undetermined species, putting forward the suggestion that these eggs belong to some Murænoid. This matter has been investigated by myself, and I have shown that the newly hatched larvæ (called “præ-larvæ” by me) derived from these eggs have essentially the character of Leptocephali.

The life history of the Murænoids, leaving aside for the present the Common Eel, is as follows:—Females can only mature in very profound depths of the sea, that is to say, at least a depth of 500 metres. This fact I established by finding well-known deep-sea fishes together with Leptocephali, ripe Murænæ, and quite ripe eels (see below). The females of those species which do not live at this depth must therefore migrate to it. The male, however, can mature at a smaller depth, and therefore they migrate into the greater depth when they are already mature. Fertilisation takes place at great depths; the eggs float in the water; nevertheless they remain at a great depth in the sea, and only exceptionally, for unknown reasons, some of them mount to the surface.

From the egg issues rapidly a præ-larva, which becomes a larva (Leptocephalus) with the anus and the urinary opening near the tip of the tail. The larva then becomes a hemi-larva, the two apertures just named moving their position towards the anterior part of the body, which becomes thickened and nearly round. By further change the hemi-larva assumes the definitive or adult form. The

larva, as well as the hemi-larva, shows a length of body much greater than that exhibited by the young Murænoid of adult form into which they are transformed. By keeping specimens in an aquarium, I was able to establish a diminution of more than 4 cm. during the metamorphosis. With regard to the greatest length which the larva can attain in a given species, and the amount of diminution which accompanies metamorphosis, there are great individual variations.

The history of the Common Eel, to which I am now about to refer, is very similar to that given above for the other Murænoids. The Common Eel (*Anguilla vulgaris*) undergoes a metamorphosis, and before it assumes the definitive adult form it presents itself as a *Leptocephalus*, which is known as *Leptocephalus brevirostris*. This *Leptocephalus* was discovered in the Strait of Messina many years ago. A specimen was also captured by the "Challenger," and another specimen was taken by the Zoological Station of Naples in the Strait of Messina. This form is occasionally carried to the surface by currents. By exception, in the month of March, in the year 1895, we captured several thousands of them in one day, but the best way to secure this *Leptocephalus* (and a very easy one) is to open the intestine of the *Orthogoriscus mola*, a fish which is common in the Strait of Messina, and in it one is certain to find a very large number of specimens. It must be observed that *Orthogoriscus mola* is a deep-sea fish. The specimens of *Leptocephalus brevirostris* found in the intestine of *Orthogoriscus* are more or less altered by digestion. Those specimens of *Leptocephalus brevirostris* which are taken near the surface in the open sea are in a better state of preservation, but, unfortunately, these also frequently have the epidermis injured so that they cannot maintain their life in an aquarium for more than a few days; they live long enough, however, to allow us to observe that it is their habit to conceal themselves in the sand or in the mud as the Common Eel (*Anguilla*) does. Here it is to be noted that the various forms of *Leptocephali* have habits resembling those of the Murænoids to which they belong, i.e., they dig into the sand or abstain from doing so according as the adult form has or has not this habit.

I now pass on to the characters of *Leptocephalus brevirostris*. I give them here in the same order as I shall use in my larger memoir. The length varies from 77—60 mm., the same extent of variation as observed in other Murænoids. The caudal fin tends to assume the form which it has in the Elver* or young *Anguilla*. It is to be noted that in other *Leptocephali* the caudal fin also tends always to exhibit the adult form. The lower jaw projects sometimes more than the

* The word "Elver" is used in this paper in its strict sense, viz., for the young form of *Anguilla vulgaris* as taken when ascending rivers in vast numbers.

upper jaw, as in *Anguilla*. The margin of the mouth is wide, as in *Anguilla*. The tongue is free, as in *Anguilla*. On the other hand, the youngest elvers which I have observed, have smaller eyes than *Leptocephalus brevirostris*, and this need not surprise us since we know that in other species of Murænoids the diminution of the eyes occurs during the metamorphosis. The nostrils are separated from one another, the anterior tubes are relatively at a considerable distance from the tip of the snout and from the rim of the mouth. They are in a position in which they are observed in many other Leptocephali, which are destined to transform themselves into adult forms having the anterior nostrils in nearly the same position as in the Common Eel. The posterior nostrils, on the contrary, are not tube-like, and are in the same position as those occupied in the adult *Anguilla*. It is worth remarking that in other Leptocephali also the posterior nostrils have already assumed the adult position when the anterior ones are still far removed from it. In *L. brevirostris* I find a larval dentition, which resembles that of the other Leptocephali. In correspondence with the small size of *Leptocephalus brevirostris* the number of larval teeth is small. Researches founded, firstly, on the enumeration of the myomeres; secondly, upon the enumeration of the dorsal and ventral arches of the vertebræ of the caudal extremity (hypurals); and, thirdly, upon the enumeration of the posterior spinal ganglia, lead with great certainty to the conclusion that the *Leptocephalus brevirostris* is the larva of a Murænoid, the number of whose vertebræ must lie between 112 and 117, most probably 114 or 115. Such a Murænoid is the *Anguilla vulgaris*. The Murænoid indicated cannot be any other of those occurring in the Mediterranean, because they all have a number of vertebræ higher than 124.* Counting the myomeres in *Leptocephalus brevirostris* one finds generally only 105 complete, five others incomplete, and all the others in a state of transparency and incomplete formation. These latter are fortunately at the posterior extremity, where other criteria come to our assistance, namely, the spinal ganglia and the vertebral arches. To show how I arrive at the number of vertebræ which must be possessed by the adult individual, corresponding to a given *Leptocephalus brevirostris*, I quote the following example:—I assume that three vertebræ develop themselves in correspondence to the first four incomplete myomeres, and that 105 must develop themselves in relation to the 105 complete myomeres, that is to say, between the fourth and fifth myomeres, between the fifth and sixth, and so on, until we reach the 105th vertebra, lying between the 104th and 105th myomeres. I

* *Murænesox savanna* is said to have 109 vertebræ, but it is doubtful whether it really occurs in the Mediterranean. The position of its nostrils and the number of its branchiostegal rays render its association with *Leptocephalus brevirostris* impossible.

further conclude that seven other vertebræ are developed at the caudal extremity, as indicated by the number of vertebral arches and the spinal ganglia in that region. We count, therefore, in all 115 vertebræ, and this is the number which can be easily seen in many specimens of *Anguilla vulgaris*.

Here I must particularly insist that I have ascertained in an absolute manner that during the metamorphosis of the Murænoids, the number neither of the myomeres nor of the vertebral arches, nor of the spinal ganglia is subjected to any change. The hypurals of *Leptocephalus brevirostris* are precisely the same as in the elver of *Anguilla vulgaris*. The last hypural which is fused with the urostyle may present itself as a single piece, or may be more or less cleft. These are variations which are met with also in the elver. Just as in the elver, the last hypural but one is always extensively cleft, or, if the expression is preferred, doubled. To the last hypural correspond five rays, whilst four correspond to the last but one, and one to the last but two, the whole structure being identical with that found in the elvers of *Anguilla vulgaris*. Of these ten rays, the eighth, seventh, and sixth are bifid, both in *Leptocephalus brevirostris* and in the elvers of *Anguilla vulgaris*. In the pectoral fin of *Leptocephalus brevirostris* the definitive rays can be observed, and these are of the same number as in the elvers of *Anguilla vulgaris*. *Leptocephalus brevirostris* is transparent, and has colourless blood. The red corpuscles are wanting, but there are present so-called "blood-plates" ("Blutplättchen" in German) similar to those of the inferior vertebrates. The bile is also colourless. This fact is observed in all the other Leptocephali. *Leptocephalus brevirostris* is, however, the only one which is free from all pigmentation. Correspondingly, the Common Eel is the only species of Murænoid which at the close of metamorphosis is devoid of all trace of larval pigmentation. It was this observation which first led us to the discovery of the relations between *Leptocephalus brevirostris* and *Anguilla vulgaris*.

In making transverse sections of *Leptocephalus brevirostris*, I found other characters which confirm the relation between it and the Common Eel; for instance, the branchiostegal rays are ten to eleven in number, as is also observed in the elvers of *Anguilla vulgaris*. In the Common Eel the well-known lateral branch of the fifth pair of the cranial nerves exists. It is also found in *Leptocephalus brevirostris*. This lateral branch could not be found by Dr. Calandruccio in the other common Murænoids of Sicily, and is wanting also in the other Leptocephali.

The mucous-canal-system (sensory canals) in the head are already developed, partially, in *Leptocephalus brevirostris*, and are incompletely developed in the elver. As in the elver, so in *Leptocephalus brevirostris*, the pyloric cæca are wanting. The blind extremity of

the stomach and the incompletely developed swim-bladder, which is as yet free from contained gas, are present both in *Leptocephalus brevirostris* and in the elver of *Anguilla vulgaris*. The pronephros is in active function as in the other Leptocephali. The Malpighian glomerules of the kidney (mesonephros) are lobed as in the eel, and their number corresponds with that observed in the Helmichthys stage, of which I will speak further on. The genital gland, not yet sexually differentiated, is almost identical with that of the same stage. In short, it may be said that the whole organisation of *Leptocephalus brevirostris* corresponds with the organisation of the Common Eel, if we make allowance for those changes, which are observed in the matamorphosis of the other species of Murænoids, such as reduction of the pancreas and of the liver, disappearance of the proto-skeleton, complication of the musculature, increase in size of the cerebellum, loss of the larval teeth, development of the definitive teeth, &c.

From the description of these Leptocephali I must pass on, briefly, to speak of the stages nearer to the condition of the elver. I am, however, obliged to leave a break in the series, which, however little its significance, yet certainly will make some impression on the minds of those who do not realise with what caution I have formed my conclusions. I must confess that since I have learnt how difficult it is to procure an entire series of the development of a Murænoid, I am more astonished at being able to recognise a single stage in the development of a given species than at not finding the whole series. I must point out that the break in my series of the development of *Anguilla vulgaris* would have been much smaller if I could have persuaded myself to kill and preserve one of the hemi-larvæ which I happened to meet with at the end of the year 1892. They were really transitional stages between *Leptocephalus brevirostris* and that stage which I shall describe further on. I published this fact in a preliminary note in the month of May, 1893. They were transparent with almost colourless blood, without any trace of pigmentation except at the eyes, and had lost all the larval teeth, whilst they possessed already very few and very minute teeth of the definitive series. The body was thickened, and already showed the cylindrical form. They measured little less than 8 cm. In short, they were *Leptocephalus brevirostris* on the way to transformation into *Anguilla vulgaris*. As a matter of history they actually did transform themselves in my aquarium with the usual diminution in their dimensions, and subsequently proceeded to increase in bulk.* The metamorphosis took place as usual without the animal taking in any

* The fact that I actually have obtained in an aquarium the transformation of *L. brevirostris* into *Anguilla vulgaris* is of prime importance. The time occupied was one month.

nourishment whatever. The resumption of growth was accompanied by a resumption of feeding. Unfortunately, I had no other individuals of this stage.

The stage which I now pass on to describe can be obtained during the winter in the sea. I have never found them at the mouths of rivers. The length varies from 54 to 73 mm. Most individuals measured about 65 mm. The body is relatively longer than in the elver. It is also relatively deeper, as in *Leptocephalus*. We are reminded of *Leptocephalus* also by the pigment of the eye, the vitreous transparency of the body, the swim-bladder being indistinguishable in the living animal, and the absence of all larval pigmentation. The blood is slightly coloured, and the bile is already green. Slight pigmentation can be seen along the central nervous system, and at the middle part of the caudal fin. This commencement of the definitive or adult pigmentation in the regions named before it occurs in any other part is also seen in other Murænoids. The definitive teeth are very minute, and few in number. The intestine contains no food. After what I had observed in the other Murænoids, the simple observation of the barely indicated teeth, and of the absence of aliment in the gut, would have been sufficient to convince me that the stage now under notice must be preceded by a *Leptocephalus* phase. Indeed, if we did not admit such a preceding history, we could not understand how this little fish could have attained such a size without acquiring well developed teeth, and without nourishing itself.

In conclusion, no one would hesitate, even not knowing *Leptocephalus brevirostris*, to refer the stage now under discussion to a Murænoid about to complete its *Leptocephalus* metamorphosis, were it not for the fact that there has been so much question concerning the reproduction of the Common Eel, and that so many capable observers have failed in dealing with it, that every new observation is received with scepticism. The stage of which I am now speaking, in the hands of a pure systematist, would probably be described as a *Helmichthys*, a genus established for certain forms of *Leptocephali* far advanced in transformation.

The next forms to which I have to refer are captured in the course of migration from the sea into fresh water. When kept in an aquarium they assume the characters of the elver, diminishing more or less in volume, and without nourishing themselves. The elvers of the Common Eel can present themselves in stages differing little from that last described, as well as in a form which has already developed the full pigmentation of the adult. Even those which most resemble the preceding stage always have a character which distinguishes them easily, namely, the presence of definitive pigment, more or less superficially placed on the head, and not to be

confounded with the pigment round the posterior extremity of the brain, which latter is already present in the preceding stage. In specimens taken at the mouths of rivers this more or less superficial pigment was, so far as I could ascertain, always present.

As the pigmentation develops itself, the little eel gradually undergoes a diminution in all its dimensions. It results from my measurements, that the fully pigmented elver has an average length of 61 mm., while for the more or less colourless elver the average length is 67 mm. I found pigmented elvers which were reduced in length to 51 mm., a size which I never observed in those elvers in which the development of pigment had not taken place.

The facts which I have stated demonstrate that the eel goes through a metamorphosis, and that *Leptocephalus brevirostris* is its larva. Some further considerations remain to be given, although I believe that zoologists will not consider the question still an open one after the record of facts given above—facts, which anyone may verify by examining the material which is preserved in my hands. Many to whom I have related my discovery of the history of the Common Eel have objected that eels are found almost everywhere, whilst *Leptocephalus brevirostris* is limited to Messina. In reply, I must say that, first of all, it is not true that *Leptocephalus brevirostris* is limited to Messina; secondly, that at Messina there are special currents, which tear up the deep-sea bottom which everywhere else is inaccessible; thirdly, although it is true that on the coasts of many countries where *Anguilla vulgaris* is found, no one has ever seen a *Leptocephalus brevirostris*; it is also true that in no country, not even in those where eels are abundant, has anyone ever seen an eel of less than 5 cm. in length. Since it has to be admitted that no one knows the eel before it arrives at the length of 5 cm., there is no greater difficulty in supposing that during this unknown period the eel passes through a *Leptocephalus* stage than in supposing that it does not do so. The critical study of the literature of this subject, and a great many continued observations, have occupied me for many years, and have been undertaken just in those places where young eels are to be found. They enable me, from my own studies, to affirm with assurance that young eels with the definitive adult form do not exist of less than 5 cm. in length.

From the study of the memoir of Raffaele on pelagic eggs, I have come to the conclusion that the eggs of his undetermined species No. 10, having a diameter of 2.7 mm. and differing from all the others in the absence of oil globules,* must belong to the *Anguilla*

* Renewed researches have convinced me that this egg is that of *Anguilla vulgaris*. There is, however, another egg belonging to an undetermined Muræoid which is devoid of oil-drops, and can easily be confused with the true eggs of *Anguilla*.

vulgaris, because from them Dr. Raffaele obtained præ-larvæ which had only forty-four abdominal myomeres. I endeavoured for two years in vain to study these eggs at the Zoological Station of Naples. I found only a few of them, and these died prematurely.

In another point my researches have yielded a very interesting result. As a result of the observations of Petersen, we know now that the Common Eel develops a bridal coloration or "mating habit," which is chiefly characterised by the silver pigment without trace of yellow, and by the more or less black colour of the pectoral fin, and finally by the large eyes. Petersen inferred that this was the bridal coloration from the circumstance that the individuals exhibiting it had the genital organs largely developed, had ceased to take nourishment, and were migrating to the sea. Here Petersen's observations cease and mine begin. The same currents at Messina which bring us the *Leptocephali* bring us also many specimens of the Common Eel, all of which exhibit the silver coloration. Not a few of them present the characters described by Petersen in an exaggerated condition, that is to say, the eyes are larger and nearly round instead of elliptical, whilst the pectoral fins are of an intense black. It is worth noting that in a certain number of them the anterior margin of the gill slit is intensely black, a character which I have never observed in eels which had not yet migrated to the sea, and which is wanting in the figures and in the originals sent to me by Petersen himself. Undoubtedly the most important of these changes is that of the increase of the diameter of the eye, because it finds its physiological explanation in the circumstance that the eel matures in the depths of the sea. That, as a matter of fact, eels dredged from the bottom of the sea have larger eyes than one ever finds in fresh-water eels, I have proved by many comparative measurements, made between eels dredged from the sea bottom and others which had not yet passed into the deep waters of the sea. Thus, for instance, in a male eel taken from the Messina currents and having a total length of $34\frac{1}{2}$ cm., the eye had a diameter, both vertical and transversal, of 9 mm., and in another eel of $33\frac{1}{2}$ cm., the same measurement was recorded. In a female eel, derived from the same source and purchased in the market, whose length was $48\frac{1}{2}$ cm., the vertical diameter of the eye was 10 mm., and the transversal diameter rather more than 10 mm. These are not the greatest dimensions which I observed, and I conclude from these facts that the bridal habit described by Petersen was not quite completed in his specimens, and that it becomes so only in the sea and at a great depth. In relation to these observations of mine stands the fact that the genital organs in the eel taken in the Messina currents are sometimes more developed than in eels which have not yet entered the deep water. Thus it has happened that male individuals have occurred showing

in the testes here and there knots of spermatozoa. These spermatozoa are similar to those of the *Conger vulgaris*, and must be considered as ripe. As is well known, so advanced a stage of sexual maturity has never before been observed in the Common Eel. This appears to be due to the fact that the males hitherto examined had not yet migrated into the deep water of the sea.

Eels with big eyes taken from the depths of the sea were, before the above facts were known, described as a distinct species under the name of *Anguilla bibroni* (Kaup) and of *Anguilla kieneri* (Kaup), not to be confounded with *Anguilla kieneri* (Günther), which is a synonym of *Lycodes kieneri*.

In certain cloacæ of ancient Rome which to-day are disused and contain pure water, remarkable eels are found of a length of from 20—30 cm. of a grey colour, without trace of yellow, of male and female sex, with enormous eyes and with more or less rudimentary genital organs. They are individuals which, confined in a place without light, have acquired prematurely one of the characters of the bridal habit without a corresponding development of the genital organs. These individuals are probably incapable of ulterior development, as the condition of their genital organs seems to demonstrate.

Under the name *Anguilla kieneri* (Kaup) there have probably been included some individuals which had acquired big eyes under conditions similar to those described for the eels of these Roman cloacæ. From these and similar observations it clearly results that all the European eels must be included under a single species, and this is an important fact from another point of view, namely, that it destroys an objection which might be raised against my conclusion with regard to the development of *Anguilla vulgaris* from *Leptocephalus brevirostris*, namely, the objection that *Leptocephalus brevirostris* belongs not to *Anguilla vulgaris*, but to *Anguilla kieneri*, or to *Anguilla bibroni*.

To sum up, *Anguilla vulgaris*, the Common Eel, matures in the depths of the sea, where it acquires larger eyes than are ever observed in individuals which have not yet migrated to deep water, with the exception of the eels of the Roman cloacæ. The abysses of the sea are the spawning places of the Common Eel: its eggs float in the sea water. In developing from the egg, it undergoes a metamorphosis, that is to say, passes through a larval form denominated *Leptocephalus brevirostris*. What length of time this development requires is very difficult to establish. So far we have only the following data:—First, *Anguilla vulgaris* migrates to the sea from the month of October to the month of January; second, the currents, such as those of Messina, throw up, from the abysses of the sea, specimens which, from the commencement of November to the end of July,

are observed to be more advanced in development than at other times, but not yet arrived at total maturity; third, eggs, which according to every probability belong to the Common Eel, are found in the sea from the month of August to that of January inclusive; fourth, the *Leptocephalus brevirostris* abounds from February to September. As to the other months, we are in some uncertainty, because during them our only natural fisherman, the *Orthogoriscus mola*, appears very rarely; fifth, I am inclined to believe that the elvers ascending our rivers are already one year old, and I have observed that in an aquarium specimens of *L. brevirostris* can transform themselves into young elvers in one month's time.

“Total Eclipse of the Sun, 1896.—The Novaya Zemlya Observations.” By Sir GEORGE BADEN-POWELL, K.C.M.G., M.P. Communicated by J. NORMAN LOCKYER, C.B., F.R.S. Received November 19,—Read November 19, 1896.

(Abstract.)

The author gives an account of the circumstances under which it became desirable to fit out an expedition to observe the eclipse in Novaya Zemlya, and the arrangements made to convey it by his yacht “Otaria.”

Details are given of the observing station, the erection of the different instruments, and the scheme of work.

The valuable spectroscopic results obtained are still under process of being worked out; but the coronagraph results are reported in detail, and copies of the chief photographs are appended. The meteorological and other conditions during the eclipse are duly recorded.

“Preliminary Report on the Results obtained with the Prismatic Camera during the Eclipse of 1896.” By J. NORMAN LOCKYER, C.B., F.R.S. Received November 17,—Read November 19, 1896.

(Abstract.)

The author first states the circumstances under which Sir George Baden-Powell, K.C.M.G., M.P., with great public spirit conveyed an eclipse party to Novaya Zemlya in his yacht “Otaria,” to which party was attached Mr. Shackleton, one of the computers employed by the Solar Physics Committee.

The prismatic camera employed, loaned from the Solar Physics

Observatory, was carefully adjusted before leaving England, and a programme of exposures was drawn up based upon the experience of 1893. As the station occupied lay at some distance from the central line, this programme was reduced by Mr. Shackleton.

Two of the photographs obtained are reproduced for the information of other workers, as some time must elapse before the discussion of all the results can be completed. This discussion and Mr. Shackleton's report on the local arrangements and details of work, are promised in a subsequent communication.

The lines photographed in the "flash" at the commencement of totality—happily caught by Mr. Shackleton—the wave-lengths of which lines have been measured by Dr. W. J. S. Lockyer, show interesting variations from those photographed by Mr. Fowler in the cusp during the eclipse of 1893.

With the exception of the lines visible in the spectra of hydrogen and helium, and the longest lines of many of the metallic elements, considerable differences of intensity from the lines of Fraunhofer are noticeable.

The coronal rings have been again photographed, and the results of 1893 have been confirmed.

November 26, 1896.

Sir JOSEPH LISTER, Bart., President, in the Chair.

Dr. George Murray and Professor Karl Pearson were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair, and the list of Officers and Council nominated for election was read as follows:—

President.—Sir Joseph Lister, Bart., F.R.C.S., D.C.L.

Treasurer.—Sir John Evans, K.C.B., D.C.L., LL.D.

Secretaries.—{ Professor Michael Foster, M.A., M.D.
{ Professor Arthur William Rücker, M.A., D.Sc.

Foreign Secretary.—Edward Frankland, D.C.L., LL.D.

Other Members of the Council.—Professor William Grylls Adams, M.A.; Professor Thomas Clifford Allbutt, M.D.; Professor Robert

Bellamy Clifton, M.A.; William Turner Thiselton Dyer, C.M.G.; Professor James Alfred Ewing, M.A.; Lazarus Fletcher, M.A.; Walter Holbrook Gaskell, M.D.; Professor Alfred George Greenhill, M.A.; William Huggins, D.C.L.; Professor Charles Lapworth, LL.D.; Major Percy Alexander MacMahon, R.A.; Professor Raphael Meldola, F.C.S.; Professor William Ramsay, Ph.D.; The Lord Walsingham, M.A.; Professor Walter Frank Raphael Weldon, M.A.; Admiral William James Lloyd Wharton, C.B.

The following Papers were read:—

- I. "Mathematical Contributions to the Theory of Evolution. On Telegony in Man, &c." By KARL PEARSON, F.R.S., University College, with the assistance of Miss ALICE LEE, Bedford College, London.
 - II. "On the Magnetic Permeability of Liquid Oxygen and Liquid Air." By J. A. FLEMING, M.A., D.Sc., Professor of Electrical Engineering in University College, London, and JAMES DEWAR, LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution.
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"Mathematical Contributions to the Theory of Evolution. On Telegony in Man, &c." By KARL PEARSON, F.R.S., University College, with the assistance of Miss ALICE LEE, Bedford College, London. Received August 27,—Read November 26, 1896.

(1) The term telegony has been used to cover cases in which a female A, after mating with a male B, bears to a male C offspring having some resemblance to or some peculiar characteristic of A's first mate B. The instances of telegony usually cited are (i) cases of thoroughbred bitches when covered by a thoroughbred dog, reverting in their litter to half-breds, when they have been previously crossed by dogs of other races. Whether absolutely unimpeachable instances of this can be produced is, perhaps, open to question, but the strong opinion on the subject among dog-fanciers is at least remarkable; (ii) the case of the quagga noted by Darwin (see 'Origin of Species,' 4th edition, p. 193), and still more recently (iii) a noteworthy case of telegony in man cited in the 'British Medical Journal' (see No. 1834, February 22, 1896, p. 462).

In this latter case a very rare male malformation, which occurred in the male B, was found in the son of his widow A, by a second husband C. Here, as in the other cases cited, a question may always be raised as to the possibly unobserved or unknown occurrence of the

characteristic in the ancestry of either A or C, or again as to the chance of the characteristic arising as a congenital sport, quite independently of any heredity. It seems unlikely that the observation of rare and isolated cases of asserted telegony will lead to any very satisfactory conclusions, although a well-directed series of experiments might undoubtedly do so. On the other hand, it is not impossible than an extensive and careful system of family measurements might bring to light something of the nature of a telegonic influence in mankind.

If such a telegonic influence really exists, it may be supposed to act in at least two and, very possibly, more ways.

(a) There may be in rare and isolated cases some remarkable change produced in the female by mating with a particular male, or some remarkable retention of the male element.

(b) There may be a gradually increasing approximation of the female to the male as cohabitation is continued, or as the female bears more and more offspring to the male.

It is extremely unlikely that any system of family measurements would suffice to bring out evidence bearing on (a). On the other hand, a closer correlation between younger children and the father, and a lesser correlation between younger children and the mother, as compared with the correlation between elder children and their parents might, perhaps, indicate a steady influence like (b) at work in mankind. Shortly, such measurements might suffice to answer the question as to whether younger children take more after their father and less after their mother than elder children. Without hazarding any physiological explanation as to the mode in which telegonic influence can or does take place, we may still hope to get, at any rate, negative evidence as to a possible steady telegonic influence by an investigation of suitable family measurements.

(2) Unfortunately, the collection of family data is by no means an easy task, and to procure those head-measurements, which, I think, would be most satisfactory for the problem of heredity, would require a large staff of ready assistants, and could only be undertaken on the necessary scale by the action of some scientific society or public body. The data concerning 800 to 900 families which have been recently collected for me deal only with stature, span, and arm-length, which are measurable with more or less accuracy by the untrained observer, and are only suitable for more or less rough appreciations of hereditary influence. The numbers in each family measured were strictly limited, in order to remove the influence of reproductive selection from the determination of the correlation between parents and children, and the result of this limitation has been that comparatively few couples of elder and younger brothers, and of elder and younger sisters are available. They were, indeed, collected in the

first place with a view to the problem of heredity in the direct line, and with no thought of their throwing any light on the problem of telegony. That steady telegonic influence might be deduced from such family data has only recently occurred to me, and I should now hesitate to publish any conclusions on this subject, based on somewhat mixed and sparse returns, did I not consider that it may be a long time before more extensive returns are available, and that the publication of this method of dealing with telegony may induce others to undertake the collection of a wider range of material.

My own 800 family data cards did not provide a sufficiently large number of either brother-brother or sister-sister couples to give a strong hope of a difference between the correlation coefficients sufficiently large as compared with its probable error to base any legitimate conclusion upon. I, therefore, again borrowed from Mr. Galton his 200 family data returns, and from these 1,000 families was able to select 385 brother-brother pairs and 450 sister-sister pairs. In these statistics each individual is only included in one pair, and the difference in age between the elder and younger members of each pair differs very widely from pair to pair. In some cases there may be several years between the ages and several intervening children; in others the members of the pair may be successive children following each other in successive years. In each case all we can say is, that if there be a steady telegonic influence, the relation of the elder member to the parent will weigh down the same scale, and in the final result we ought to find a distinctly greater or less correlation, as the case may be. I think a more serious objection to the data than the variation in the number of years between fraternal pairs is the mixture I have made of data collected at different periods and in somewhat different manners. My own data are drawn, I think, from a wider class of the community than Mr. Galton's. They are not exclusive of his class, but, I think, cover his class, and go somewhat further down in the social scale. They suffice to show that the means and variations change considerably from one social stratum to another, and what is still more important that the Galton-Functions or coefficients of correlation for heredity are far from being constant even within the same race, as we pass from one rank of life to a second. Thus, my means for stature in the case of both fathers and mothers are upwards of $\frac{1}{2}$ in. less than Mr. Galton's, but my means agree fairly well with his results in the case of both sons and daughters. There are also good agreements and somewhat puzzling disagreements not only in the variations, but, above all, in the coefficients of correlation for heredity. I reserve for the present the full discussion of my heredity data, but I wish it to be quite understood that my conclusions in this paper are based, not upon the best possible data, *e.g.*, measurements made on one class of the com-

munity under one system, but upon all the data which, for some time to come, appear likely to be available. These data are neither quantitatively nor qualitatively ideal, but, on the other hand, they must be given a reasonable amount of weight in considering whether, at any rate in the case of one organ—stature,—any steady telegonic influence can be traced in man.

The reduction from the family measurement-cards, the formation of the eight correlation tables, and the calculation of both variation and correlation coefficients have been undertaken by Miss Alice Lee of Bedford College,—a task requiring much labour and persistency. I have independently verified, and in some minor points corrected her calculations, as well as added the probable errors of the constants determined.

(3) The following are the means and standard-deviations with their probable errors for the various groups.

Table I.—Stature of Families in Inches.

Class.	Number.	Mean.	Standard deviation.
Fathers of sons	385	$68\cdot5740 \pm 0\cdot0878$	$2\cdot5554 \pm 0\cdot0621$
Elder sons	"	$69\cdot1494 \pm 0\cdot0913$	$2\cdot6550 \pm 0\cdot0645$
Younger sons	"	$69\cdot1948 \pm 0\cdot0933$	$2\cdot7128 \pm 0\cdot0659$
Mothers of sons	"	$63\cdot3078 \pm 0\cdot0854$	$2\cdot4848 \pm 0\cdot0604$
Fathers of daughters	450	$68\cdot3344 \pm 0\cdot0878$	$2\cdot7605 \pm 0\cdot0621$
Elder daughters	"	$63\cdot9244 \pm 0\cdot0823$	$2\cdot5878 \pm 0\cdot0582$
Younger daughters	"	$64\cdot2200 \pm 0\cdot0794$	$2\cdot4985 \pm 0\cdot0562$
Mothers of daughters	"	$63\cdot1794 \pm 0\cdot0758$	$2\cdot3827 \pm 0\cdot0536$

All the quantities have here been calculated precisely as in my third memoir on the mathematical theory of evolution (see 'Phil. Trans.,' A, vol. 187, pp. 270—271). In this case, however, no child is included twice as a child, and parents are not weighted with their offspring. Thus reproductive selection is not allowed to influence the results.

It will be seen that the probable errors of the means and standard deviations are, as in the former paper, too large to allow of absolutely definite conclusions when those conclusions are not supported by a continuous change of values, or directly verified by the numbers of the earlier memoir. But one or two such conclusions may be drawn, and I will note them before passing to correlation.

(i) The law of sexual interchange referred to in my former paper (p. 274) is confirmed with greater uniformity. Fathers of sons are sensibly less variable than fathers of daughters, and mothers of daughters are sensibly less variable than mothers of sons. In other

words, to judge from stature, the exceptional parent tends to have offspring of the opposite sex.

(ii) Younger sons are taller and more variable than elder sons, and elder sons are taller and more variable than fathers.

This conclusion, although less markedly, appears in the results on pp. 270 and 281, of my former paper. It might be accounted for by :

- (a) A secular change going on in the stature of the population, and even noticeable in the difference between the stature of younger and elder sons.
- (b) A further growth of sons, and an ultimate shrinkage, which will leave them at the age of their fathers with the same mean height and variation.
- (c) Conditions of nurture on the average less favourable, and on the whole less varied in the case of elder than in that of younger children.*
- (d) Natural selection. The difference between younger and elder sons and between elder sons and fathers represents the selective death rate in man due to causes correlated with stature in the years between youth and manhood, and manhood and age. The difference is thus to be accounted for by a periodic and not a secular change.

Possibly (a), (b), (c), and (d), may all contribute to the observed results. It cannot be denied that (d) has a special fascination of its own for the student of evolution, but prolonged study of the laws of growth must precede the assertion that we have here, or in any similar case, real evidence of an actual case of natural selection.

(iii) Younger daughters are taller than elder daughters and elder daughters than mothers.

This is in complete agreement with the result for fathers and sons. Further :

Daughters, as a class are far more variable than mothers, but while in the earlier memoir younger daughters were sensibly more variable than elder daughters—and thus exactly corresponded with sons—elder daughters are in this case more variable than younger. I have been unable to find any slip in the tables or calculations, which might account for this divergence. It exceeds considerably the probable error of the observations, and is not in accordance with the general law connecting the variation of parent and offspring evidenced for both sexes in the earlier, and for sons in the present memoir—*e.g.*, the variation—whether it be due to growth-change,

* Mr. Francis Galton suggests this as a possible cause. It has, I think, to be taken in conjunction with a greater amount of parental experiment, not only in the birth, but in the nurture of the elder children.

or to selective death-rate, or to secular evolution—diminishes with age.

(4) The following are the coefficients of correlation (r) and the coefficients of regression (R) for parents and sons:

Table II.—Inheritance of Stature by Sons.

	r .	R .
Father and elder sons	$0\cdot4120 \pm 0\cdot0264$	$0\cdot4281$
Father and younger sons....	$0\cdot4170 \pm 0\cdot0262$	$0\cdot4427$
Mother and elder sons	$0\cdot4094 \pm 0\cdot0265$	$0\cdot4374$
Mother and younger sons ...	$0\cdot4111 \pm 0\cdot0264$	$0\cdot4488$

If we measure, as seems reasonable, the hereditary influence of parentage by the magnitude of the coefficient of correlation between parent and offspring, then several important conclusions may be drawn from this table.

(i) There is no sensible difference between the influences of the father on younger and on elder sons, and no sensible difference between the influences of the mother on younger and on elder sons.

If we pay attention to such slight differences as exist, there would appear, not to be an increase of paternal and a decrease of maternal influence on younger children, but an extremely slight increase of both. In other words, so far as stature in sons is concerned, judged by correlation: *No steady telegonic influence exists.*

(ii) There is a very slight prepotency of the father over the mother in the case of both younger and elder sons; a prepotency which will be slightly magnified when account is taken of the absolute stature of the two parents.

But the great prepotency of paternal inheritance noticed in the former memoir is not confirmed. The co-efficients of maternal inheritance have been increased by more than 30 per cent. (from $0\cdot293$ to $0\cdot410$), while those of paternal inheritance ($0\cdot396$ as compared with $0\cdot414$) have remained almost stationary. This result seems to show the want of constancy of the Galton's functions for heredity within the same race. An explanation on the ground that the present statistics embrace a wider range of the community than the earlier, and possibly a more closely correlated class,* fails, at any rate in part, owing to the sensible constancy of the paternal correlation. The main difference of course between the present and the former statistics is the exclusion of the influence of reproductive

* I have pointed out (*loc. cit.*, p. 284) that working and lower middle class families appear to be more closely correlated than those of the upper middle class.

selection, but why should this be expected to influence only the mother? The father of many children remains equally influential, but the mother's relation is weakened when we give weight to the quantity not the relative ages of her children. This is not a steady telegonic influence, but a correlation between fertility and hereditary influence in mothers, which if it could be verified by further observation, would undoubtedly be of high significance. I would accordingly suggest as a possible law of heredity, deserving careful investigation, that: *Hereditary influence in the female varies inversely as fertility.*

In my paper on "Reproductive Selection," ('Roy. Soc. Proc.' vol. 59, p. 301), I have pointed out the important evolutionary results which flow from a correlation between fertility and any inheritable characteristic. If a law of the above character should be established after further investigation, it is conceivable that it may act as an automatic check on the extreme effects of reproductive selection.

(iii) The above results give us for practicable purposes a quite sufficiently close value of the correlation between parents and sons, when the influence of reproductive selection is excluded. Judging from stature the correlation between sons and parents is very closely given by

$$0.41 \pm 0.03.$$

The $\frac{1}{3}$, adopted by Mr. Galton, may, I think, safely be increased by 25 per cent., and further, the assumption that collateral heredity is twice as strong as direct heredity must, I hold, be finally discarded, for no determination of the former has given such a high value as 0.82.

(5) Hitherto we have regarded only the coefficients of correlation, and considered them to measure the strength of the hereditary influence, but it must be remembered that the means of elder and younger sons are not the same, and that there is another way of looking at the problem. We may ask: Do younger or elder sons differ most from the stature of their father, and is the order altered in the case of the mother?

If we neglect the influence of sexual selection (see "Contributions to Math. Theory of Evolution," III, pp. 287—8) we have, if h_f and h_m be deviations of father and mother from their means, and M_e and M_y be mean heights of corresponding fraternities of elder and younger sons in inches:

$$M_e = 69.1494 + 0.4281h_f + 0.4374h_m.$$

$$M_y = 69.1948 + 0.4427h_f + 0.4488h_m.$$

Now the ratio of the mean heights of parents is $68.5740 : 63.3078 =$

1·0832,* while the ratios of 0·4374 to 0·4281 and 0·4488 to 0·4427, are only 1·0219 and 1·0139 respectively, thus there is still a slight prepotency of paternal influence on stature to be recorded. (See § (4) (ii).)

Confining our attention to the differences in stature for fathers and sons corresponding to all mothers whatsoever, we have, if D_{ef} be the difference in stature between father and corresponding fraternity of elder sons, D_{yf} between father and fraternity of younger sons :

$$D_{ef} = 0·5754 - 0·5719h_f.$$

$$D_{yf} = 0·6208 - 0·5573h_f.$$

Hence the difference between the father and fraternity of younger sons will be greater than the difference between the father and the corresponding fraternity of elder sons unless the father be 3·110 inches less, or 1·059 more than the average. But 3·11 is about 1·2 and 1·059 about 0·415 times the standard deviation of the stature of fathers, or, fraternities of younger sons are nearer in stature to their father than fraternities of elder sons in about 46 per cent. of cases.

Similarly if D_{em} , D_{ym} represent the differences of stature of mothers and fraternities of elder and younger sons respectively, we have in inches

$$D_{em} = 5·8416 - 0·5626h_m.$$

$$D_{ym} = 5·8870 - 0·5512h_m.$$

Thus fraternities of younger sons are always more divergent than fraternities of elder sons from the stature of their mothers, unless the mother be 3·982 inches less, or 10·53 inches more than the average. These are 1·6 and 4·24 times the standard deviation in stature of mothers; or, only in about 5·5 per cent. of cases are fraternities of younger sons nearer in stature to their mothers than elder sons.

Now, it is difficult to read into these results any evidence for a steady telegonic influence. It is true that the case of younger sons being more like their parents than elder sons occurs in eight times as many cases with the father as with the mother, but the broad fact remains that in more than half the cases, judged by difference of stature, the elder son is more like the father than the younger son. In fact, examined in this way by difference of stature—not an unnatural manner of first approaching the problem—the true closeness of parent and offspring appears to be quite obscured by some secular, or, at any rate, periodic (see § 3) evolution in stature between successive generations—an evolution which even makes itself felt in the interval between younger and elder sons.

* $13/12 = 1·0833$; thus these returns again confirm Mr. Galton's selection of this fraction for the sexual ratio for stature.

(6) Turning to the results for daughters, we have the following table for the coefficients of correlation and regression :—

Table III.—Inheritance of Stature by Daughters.

	<i>r.</i>	<i>R.</i>
Fathers and elder daughters.	0·4829 ± 0·0220	0·4528
Fathers and younger daughters ...	0·4376 ± 0·0236	0·3961
Mothers and elder daughters.	0·3953 ± 0·0250	0·4293
Mothers and younger daughters ...	0·4542 ± 0·0230	0·4763

These results, more numerous than those for sons, are, for reasons which I am unable to explain, much more divergent. We may note the following points :—

(i) There is a sensible difference between the coefficients of correlation for either parents with younger and elder daughters. Thus, the difference of the coefficients for fathers with elder and younger daughters is 0·0453, and the probable error of this only 0·032; while for mothers the corresponding difference is 0·0589, and the probable error of the difference only 0·0328. The difference, however, is in the *opposite* sense. We are thus face to face with an increasing maternal and a decreasing paternal influence on the stature of daughters. In other words, our statistics are entirely opposed to any steady telegonic influence on the stature of daughters. If such a thing were conceivable, we should be confronted with the case of the mother influencing the father, the reverse of telegony.

(ii) The mean correlation of fathers and daughters is very slightly higher than that of mothers and daughters (0·4602 as compared with 0·4247). Thus, to judge by the mean coefficients of correlation, the father is slightly more prepotent than the mother in heredity. The mean coefficients of regression are for fathers 0·4244, and for mothers 0·4528, or in the ratio of 1 : 1·067, but the ratio of the paternal to the maternal stature is 1·083, or this slight prepotency is still preserved if we judge the matter by regression coefficients. Again, we notice an immense increase (0·2841 to 0·4247) in the correlation between mothers and daughters when we compare the present results with those of my earlier memoir. As an explanation of this, I have already suggested the possibility of a law exhibiting a relation between fertility and hereditary influence in mothers (§ 4 (ii)).

(iii) The mean coefficient of correlation in stature between either parent and a daughter may be taken to be—

$$0\cdot44 \pm 0\cdot02.$$

Thus, it does not differ very widely from the value suggested (0.41) for sons, but is even further removed from the value (0.33) at first determined by Mr. Galton.

The greater correlation between sons and both parents noticed in my first memoir is not borne out by the present statistics; the advantage is now—it is true to a much less extent—with daughters.

On the whole, I am not well satisfied with these results for daughters. I can see no persistent source of error in the method of collecting the observations, nor can I find any mistake in the calculations. I can only trust that more elaborate returns and measurements of other characteristics may some day throw light on what now appear to be anomalies.

(7) Finally, I may just notice what conclusions are to be drawn, if we pay attention to the absolute difference in stature between parents and daughters. Let δ_{em} and δ_{ym} be the differences in stature between elder daughters and mothers, and younger daughters and mothers respectively, then in inches we have for the corresponding arrays:

$$\delta_{em} = 0.7450 - 0.5707h_m.$$

$$\delta_{ym} = 1.0406 - 0.5237h_m.$$

Thus, arrays of younger daughters differ more from their mothers in stature than arrays of elder daughters, if the mothers be more than 6.29 in. below the mean or more than 1.63 in. above the mean, or if their deviations are not within the limits of about -2.64 and 0.68 times the standard deviation of mothers. This gives us about 74 to 75 per cent. of elder sisters nearer in stature to their mothers than younger sisters.

If δ_{fe} , δ_{fy} be the stature differences for fathers and daughters, we have:

$$\delta_{fe} = 4.4100 - 0.5472h_f.$$

$$\delta_{fy} = 4.1144 - 0.6039h_f.$$

Here, so long as the father lies between 5.21 in. less and 7.41 in. more than the average, the array of younger daughters will more nearly approach him in stature than the array of elder daughters. These limits correspond to 1.89 and 2.68 times the standard deviation of fathers. Accordingly, about 96 to 97 per cent. of younger sisters are closer in stature to their fathers than elder sisters. Thus, if we had started the discussion of the problem from a consideration of the relative nearness in stature of daughter to father and mother, we should have found that a great majority of younger sisters were nearer to their fathers than their elder sisters, and a considerable majority of elder sisters nearer to their mother than their younger sisters. We might then have concluded that there were substantial

grounds for inferring the existence of a telegonic influence. But it is clear that if there be anything of the nature either of a periodic or of a secular change in stature going on, then since men are taller than women, any group of younger women will appear closer to their fathers than to their mothers, when compared with a group of elder sisters. Thus, no legitimate argument as to a telegonic influence can be based on such a result. I have purposely considered this method of approaching the problem, because it is the method which first occurred to me, as it probably may do to others. It can very easily, however, lead to our mistaking for a real telegonic influence an effect of periodic or secular evolution, or, indeed, of different conditions of nurture.

(7) In conclusion, we may, I think, sum up the statistics discussed in this paper as follows:—

- (i) So far as stature is concerned there is no evidence whatever of a steady telegonic influence of the male upon the female among mankind.
- (ii) It is improbable that the coefficients of correlation which measure the strength of heredity between parents and offspring are constant for all classes even of the same race.

For stature in the case of parents and offspring of both sexes, the value 0.42, or say $\frac{3}{7}$, may be taken as a fair working value, until more comprehensive measurements are made. This makes hereditary influence in the direct line stronger than has hitherto been supposed.

- (iii) The divergence between the results of this memoir and that of the former memoir on “Regression, Heredity, and Panmixia” would be fairly well accounted for, if there be a hitherto unobserved correlation between the hereditary influence and the fertility of woman.

RRSL 60

“On the Magnetic Permeability of Liquid Oxygen and Liquid Air.” By J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London, and JAMES DEWAR, LL.D., F.R.S., Fullerman Professor of Chemistry in the Royal Institution, &c. Received November 20,—Read November 26, 1896.

The remarkable magnetic properties of liquid oxygen were pointed out by one of us in a communication to the Royal Society in 1891,*

* ‘Roy. Soc. Proc.’ December 10th, 1891, vol. 51, p. 24. See a letter to the President by Professor James Dewar, F.R.S.

and were subsequently described to the Royal Institution in a lecture delivered in 1892.* We have for some time past directed our attention to the question of determining the numerical values of the magnetic permeability and magnetic susceptibility of liquid oxygen, with the object of determining not only the magnitude of these physical constants, but also whether they vary with the magnetic force under which they are determined.

Although a large number of determinations have been made by many observers of the magnetic susceptibility of different liquids taken at various temperatures, difficulties of a particular kind occur in dealing with liquid oxygen. One method adopted for determining the magnetic susceptibility of a liquid is to observe the increase of mutual induction of two conducting circuits suitably placed, first in air, and then when the air is replaced by the liquid in question, the susceptibility of which is to be determined. A second method consists in determining the mechanical force acting on a known mass of the liquid when placed in a non-uniform magnetic field. Owing to the difficulty of preventing entirely the evaporation of liquid oxygen, even when contained in a good vacuum vessel, and the impossibility of sealing it up in a bulb or tube, and having regard to the effect of the low temperature of the liquid in deforming by contraction and altering the conducting power of coils of wire placed in it, it was necessary to devise some method which should be independent of the exact constancy in mass of the liquid gas operated upon, and independent also of slight changes in the form of any coils of wire which might be used in it. After many unsuccessful preliminary experiments the method which was finally adopted as best complying with the conditions introduced by the peculiar nature of the substance operated upon is as follows:—

A small closed circuit transformer was constructed, the core of which could be made to consist either of liquid oxygen or else immediately changed to gaseous oxygen, having practically the same temperature. This transformer consisted of two coils, the primary coil was made of forty-seven turns of No. 12 S.W.G. wire, this wire was wound into a spiral, having a rectangular shape, the rectangular turns having a length of 8 cm. and a width of 1.8 cm. This rectangular-sectioned spiral, consisting of one layer of wire of forty-seven turns, was bent round a thin brass tube, 8 cm. long and $2\frac{1}{2}$ cm. in diameter, so that it formed a closed circular solenoid of one layer of wire. The wire was formed of high conductivity copper, doubly insulated with cotton, and each single turn or winding having a rectangular form.

The turns of covered wire closely touched each other on the inner circumference of the toroid, but on the external circumference were

* See 'Roy. Inst. Proc.,' June 10th, 1892, "On the Magnetic Properties of Liquid Oxygen." Friday evening discourse, by Professor J. Dewar, F.R.S.

a little separated, thus forming apertures by which liquid could enter or leave the annular inner core.

The nature of this transformer is shown in Fig. 1.

FIG. 1.

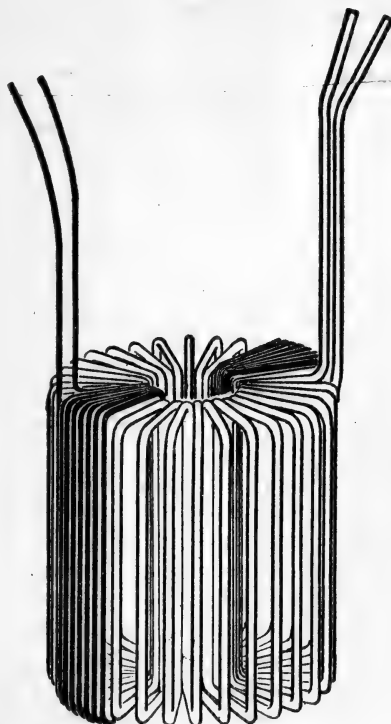


Diagram of the Closed Circuit Transformer used in the Experiments.

The mean perimeter of this rectangular-sectioned endless solenoid was $13\frac{1}{2}$ cm., and the solenoid had, therefore, very nearly 3.5 turns per cm. of mean perimeter. When immersed in liquid oxygen a coil of this kind will carry a current of 50 amperes. When a current of *A* amperes is sent through this coil the mean magnetising force in the axis of this solenoid is, therefore, represented by 4.375 times the current through the wire, hence it is clear that it is possible to produce in the interior of this solenoid a mean magnetising force of over 200 C.G.S. units. This primary coil had then wound over it, in two sections, about 400 or 500 turns of No. 26 silk-covered copper wire to form a secondary coil. The primary and secondary coils were separated by layers of silk ribbon. The exact number of turns was not counted, and as will be seen from what follows it was not necessary

to know the number. The coil so constructed constituted a small induction coil or transformer, with a closed air-core circuit, but which when immersed in a liquid, by the penetration of the liquid into the interior of the primary coil, became changed into a closed circuit transformer, with a liquid core. The transformer so designed was capable of being placed underneath liquid oxygen contained in a large vacuum vessel, and when so placed formed a transformer of the closed circuit type, with a core of liquid oxygen. The coefficient of mutual induction of these two circuits, primary and secondary, is therefore altered by immersing the transformer in liquid oxygen, but the whole of the induction produced in the interior of the primary coil is always linked with the whole of the turns of the secondary coil, and the only form-change that can be made is a small change in the mean perimeter of the primary turns due to the contraction of the coil as a whole. In experiments with this transformer the transformer was always lifted out of the liquid oxygen into the cold gaseous oxygen lying on the surface of the liquid oxygen, and which is at the same temperature. On lifting out the transformer, the liquid oxygen drains away from the interior of the primary coil, and is replaced by gaseous oxygen of very nearly the same temperature.

The vacuum vessel used had a depth of 60 cm. outside and 53 cm. inside, and an internal diameter of 7 cm. It held 2 litres of liquid oxygen when full; but, as a matter of fact, 4 or 5 litres of liquid oxygen were poured into it in the course of the experiment.

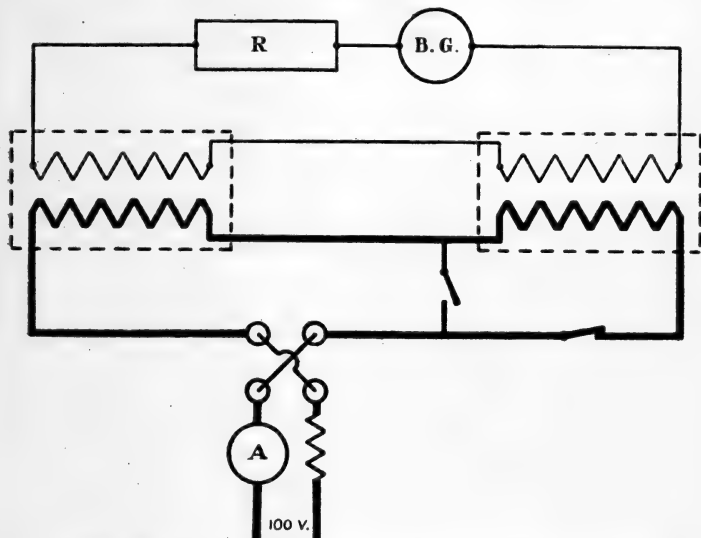
Another induction coil was then constructed, consisting of a long cylindrical coil wound over the four layers of wire, and a secondary circuit was constructed to this coil, consisting of a certain number of turns wound round the outside of the primary coil, and a small adjusting secondary coil, consisting of a thin rod of wood wound over with very open spirals of wire. The secondary turns on the outside of the primary coil were placed in series with the turns of the thin adjusting coil, and the whole formed a secondary circuit, partly outside and partly inside the long primary cylindrical coil, the coefficient of mutual induction of this primary and secondary coil being capable of being altered by very small amounts by sliding into or out of the primary coil the small secondary coil. This last induction coil, which will be spoken of as the balancing coil, was connected up to the small transformer, as just described, as follows:—

The primary coil of the small transformer was connected in series with the primary coil of the balancing induction coil, and the two terminals of the series were connected through a reversing switch and ammeter with an electric supply circuit, so that a current of known strength could be reversed through the circuit, consisting of the two primary coils in series. The two secondary coils, the one on

the transformer and the one on the balancing induction coil, were connected in opposition to one another through a sensitive ballistic galvanometer in such a manner that on reversing the primary current the galvanometer was affected by the difference between the electromotive forces set up in the two secondary coils, and a very fine adjustment could be made by moving in or out the adjusting coil of the balancing induction coil.

The arrangement of circuits is shown in fig. 2.

FIG. 2.



Arrangement of the Circuits of the Transformer and Induction Coil.

For the purpose of standardising the ballistic galvanometer employed, the primary coil of the balancing induction coil could be cut out of circuit, so that the inductive effect in the ballistic galvanometer circuit was due to the primary current of the closed circuit transformer alone. A resistance box was also included in the circuit of the ballistic galvanometer. The resistance of the ballistic galvanometer was about 18 ohms, and the resistance of the whole secondary circuit 30·36 ohms. The experiment then consisted in first balancing the secondary electromotive forces in the two coils exactly against one another, then immersing the transformer in liquid oxygen, the result of which was to disturb the inductive balance, and in consequence of the magnetic permeability of the liquid oxygen core being greater than unity, a deflection of the ballistic galvanometer was observed on reversing the same primary current. The induction

through the primary circuit of the small transformer is increased in the same proportion that the permeability of the transformer core is increased by the substitution of liquid oxygen for gaseous oxygen, and hence the ballistic deflection measures at once the amount by which the magnetic permeability of the liquid oxygen is in excess over that of the air or gaseous oxygen forming the core of the transformer when the transformer is lifted out of the liquid. As a matter of fact it was never necessary to obtain the inductive balance precisely. All that was necessary was to observe the throw of the ballistic galvanometer, first when the transformer was wholly immersed under the surface of liquid oxygen, and, secondly, when it was lifted out into the gaseous oxygen lying on the surface of the liquid, the strength of the primary current reversed being in each case the same. In order to standardise the galvanometer and to interpret the meaning of the ballistic throw, it was necessary to cut out of circuit the primary coil of the balancing induction coil, and to reverse through the primary circuit of the small transformer a known small primary current, noting at the same time the ballistic throw produced on the ballistic galvanometer, this being done when the transformer was underneath the surface of liquid oxygen. It will be seen, therefore, that this method requires no calculation of any coefficient or mutual induction, neither does it involve any knowledge of the number of secondary turns on the transformer, nor of the resistance of the secondary circuit; all that is necessary for a successful determination of the magnetic permeability of the liquid oxygen is that the secondary circuit of the transformer should remain practically of the same temperature during the time when the throw of the ballistic galvanometer is being observed, both with the transformer underneath the liquid oxygen and out of the liquid oxygen. If then the result of reversing a current of A amperes through the two primary coils in series when the secondary coils are opposed is to give a ballistic throw, D , and if the result of reversing a small current a amperes through the primary coil of the transformer alone is to produce a ballistic throw, d , then if μ is the magnetic permeability of liquid oxygen, that of the gaseous oxygen lying above the liquid and at the same temperature being taken as unity, we have the following relation:—

$$\frac{D}{\frac{A}{a} d} = \mu - 1,$$

which determines the value of μ .

Deferring for a moment the correction to be applied to determine the value of the magnetic permeability of liquid oxygen in terms of that of a vacuum, the following are the results of observation:—

OBSERVATIONS ON MAGNETIC PERMEABILITY OF LIQUID OXYGEN.

Throws of Ballistic Galvanometer. Induction Coils balanced.

Exp. I.	$\left\{ \begin{array}{l} 4.0 \text{ mm. to left} \\ 4.2 \text{ " " " } \\ 4.3 \text{ " " " } \end{array} \right\}$	<p>The transformer in liquid oxygen. Primary current = 37.8 amperes reversed through primary coils.</p>
Exp. II.	$\left\{ \begin{array}{l} 17.0 \text{ mm. to right} \\ 17.5 \text{ " " " } \\ 18.5 \text{ " " " } \end{array} \right\}$	<p>The transformer lifted out of liquid oxygen into cold gaseous oxygen at the same temperature. Primary current = 37.8 amperes reversed through primary coils.</p>
Exp. III.	$\left\{ \begin{array}{l} 3.2 \text{ mm. to left} \\ 2.5 \text{ " " " } \\ 2.8 \text{ " " " } \end{array} \right\}$	<p>The transformer in liquid oxygen. Primary current = 37.2 amperes reversed through primary coils.</p>
Exp. IV.	$\left\{ \begin{array}{l} 20.0 \text{ mm. to right} \\ 21.0 \text{ " " " } \\ 21.3 \text{ " " " } \end{array} \right\}$	<p>The transformer lifted out of liquid oxygen into cold gaseous oxygen at the same temperature. Primary current = 36.8 amperes reversed through primary coils.</p>

*Throws of Ballistic Galvanometer in Standardising Observations.
Primary Coil of Balancing Coil disconnected.*

Exp. V.	$\left\{ \begin{array}{l} 24.0 \text{ mm. to right} \\ 25.0 \text{ " " " } \\ 25.0 \text{ " " " } \\ 24.5 \text{ " " " } \\ 25.0 \text{ " " " } \end{array} \right\}$	<p>Corresponding to 0.1145 ampere reversed through primary coil of the transformer, the transformer being in liquid oxygen. The mean of these ballistic throws is the quantity denoted by d, and the current 0.1145 ampere is the a in the formula above.</p>
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Standardising Observations repeated with another Current.

Exp. VI.	$\left\{ \begin{array}{l} 58.0 \text{ mm. to right} \\ 58.0 \text{ " " " } \end{array} \right\}$	<p>Corresponding to 0.2639 ampere reversed through primary coil of transformer, the transformer being in liquid oxygen.</p>
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Throws of Ballistic Galvanometer. Induction Coils balanced.

Exp. VII.	$\left\{ \begin{array}{l} 4.0 \text{ mm. to right} \\ 4.0 \text{ " " " } \end{array} \right\}$	<p>The transformer lifted out of liquid oxygen into cold gaseous oxygen at same temperature. Primary current = 8.037 amperes reversed through primary coils.</p>
Exp. VIII.	$\left\{ \begin{array}{l} 0.4 \text{ mm. to left} \\ 0.4 \text{ " " " } \\ 0.2 \text{ " " " } \end{array} \right\}$	<p>The transformer in liquid oxygen. Current = 8.095 amperes reversed through primary coils.</p>
Exp. IX.	$\left\{ \begin{array}{l} 4.5 \text{ mm. to left} \\ 4.8 \text{ " " " } \\ 4.2 \text{ " " " } \end{array} \right\}$	<p>The transformer in liquid oxygen. Current = 28.8 amperes through primary coils.</p>

Exp. X.	{	12.0 mm. to right	}	The transformer lifted out of liquid oxygen into cold gaseous oxygen at the same temperature. Current = 28.1 amperes reversed through primary coils.
		12.0 " "		
		12.2 " "		
Exp. XI.	{	1.2 mm. to left	}	The transformer in liquid oxygen. Current = 28.1 amperes reversed through primary coils.
		1.3 " "		

Exp. XII.

The transformer in liquid oxygen.		The transformer lifted out of liquid oxygen into cold gaseous oxygen at same temperature.	
Current reversed in primary coils, in amperes.	Ballistic throw in millimetres. Deflection to the right.	Current reversed in primary coils, in amperes.	Ballistic throw in millimetres. Deflection to the right.
58.8	10.5	—	—
—	—	50.2	47.0
50.2	15.0	—	—
—	—	50.8	48.5
50.2	17.0	—	—
—	—	50.0	49.0

The above table shows the results of the observations made with the small transformer alternately placed underneath the surface of liquid oxygen, and then lifted up into the cold gaseous oxygen lying above the surface of the liquid oxygen. It will be noticed that the ballistic throws in each set of observations are not constant, but that there is a tendency, usually, for the throw to increase if repeated, whilst the transformer is still maintained in the same condition. This is in all probability due to the fact that the continued passage of the primary current heats the primary circuit of the balancing induction coil, and hence heats, also, by radiation, the secondary coil of the balancing induction coil, and, therefore, by enlarging the area of the adjusting coil, continually breaks down the inductive balance. It was found necessary, therefore, to take the observations in groups at equal intervals of time. First, a group of three observations was taken, the transformer being in liquid oxygen, the balance being, as nearly as possible, obtained. Then the transformer was lifted out of the liquid oxygen, and the ballistic throws again taken, reversing the same primary current; next again immersed in liquid oxygen, and finally once more taken out of the liquid oxygen. Taking the sets

of observations marked I, II, III, IV, the mean of the means of the three observations in Sets I and III, corrected for the variation in the primary current, were taken as the result of the measurement in liquid oxygen, and this result was then compared with the ballistic throws in Set II.

Again, the mean of the means of sets of observations II and IV, properly corrected for variation of primary current, were compared with the mean of the observations in Set III, and the result is to give the data for calculating the permeability of the liquid oxygen for a primary current through the primary coil of the transformer of about 37 amperes, corresponding very nearly to a mean magnetising force of 166 C.G.S. units. The sum or difference of these means of the throws, taken in the liquid oxygen and out of the liquid oxygen, depending on whether they are on the opposite or the same side of the zero of the scale, gives us the value of the quantity denoted by D in the Table I below, and in the formula for the value of μ .

The above sets of observations, I, II, III, and IV, refer to a primary current of about 37 amperes; but similar sets of observations were taken with a primary current of about 8 amperes, 28 amperes, and 50 amperes respectively, and the results of all these observations, which are included in the sets of observations, I to XII, above given, have been reduced in Table I below to show the magnetic permeability of the liquid oxygen corresponding to different magnetising currents. The set of observations marked Experiment V and Experiment VI in the above table of results, gives the observations for standardising the ballistic galvanometer. In the first case the primary coil of the balancing induction coil was cut out, and a primary current, having a value of 0.1145 ampere, was reversed through the primary coil of the transformer alone, and gave ballistic deflections as stated in the observations in Set V. These observations serve to standardise the galvanometer and interpret the meaning of the throw obtained when the large current is reversed through the primaries of the two induction coils, the secondaries of which are opposed. It will be noticed that one important advantage of the above-described method is that the quantity which we desired to know, viz., the amount by which the presence of the liquid oxygen increases the magnetic permeability of the core of the transformer, is the quantity which is measured directly, and that any error in the measurement of this quantity does not affect the permeability to anything like the same proportional extent. An error of about 10 per cent. in the measurement of the ballistic throw would only affect the fourth place of decimals in the number representing the permeability of the liquid oxygen.

The results of all the above observations, when reduced, are comprised in the following table:—

Table I.—Table of Results of Observations on the Magnetic Permeability of Liquid Oxygen.

A = primary current, in amperes, passing through primaries of the transformer and balanc- ing coil.	Correspond- ing mean magnetising force in C.G.S. units in primary circuit of transformer.	Total ballistic throw which would be produced if primary current of A amperes were reversed through primary of trans- former alone $= \frac{A}{a}d.$	Ballistic throw of galvanometer resulting from immersion of the transformer in liquid oxygen. Transformer and balancing induction coil being opposed $= D.$	$\mu =$ permeability calculated from $\mu - 1 = \frac{D}{\frac{A}{a}d}.$
8·037	35·2	1734	4·33	1·00250
28·13	123·0	6068	14·9	1·00246
37·8	165·4	8153	21·18	1·00260
36·8	161·0	7938	23·57	1·00297
50·5	220·9	10894	32·98	1·00304

The values of the permeability given in the foregoing table are not all of equal weight.

The calculated value of $\mu - 1$ depends upon the observed ballistic throw, and this cannot be read to a high degree of accuracy when the throw is as small as 4 millimetres. We consider that the best result is obtained by taking the mean of the values for the primary currents, 37·8, 36·8, and 50·5 amperes, and these values give $\mu = 1·00287$, with a probable accuracy of $\pm 0·0002$. This value of the permeability of the liquid oxygen corresponds to a magnetising force lying between 166 and 220 C.G.S. units. It will be seen that this method is best applicable to the determination of the permeability under large magnetising forces; and that these observations do not, in themselves, allow us to state whether the permeability is a constant for all forces, or is a function of the value of the force.

In the next place the value is a relative one. The number 1·00287 is the ratio of the magnetic permeability of liquid oxygen to that of the gaseous oxygen nearly at the same temperature resting upon the surface of the liquid. We were not able by this method to detect the difference between the permeability of the cold gaseous oxygen lying on the surface of the liquid oxygen when in quiet ebullition, and which has a temperature of about -182° C., but a density of at least three times that of oxygen at 0° C., when compared with that of gaseous oxygen at ordinary temperature, and under the normal pressure. In a very valuable memoir on the determination of magnetic susceptibilities, M. P. Curie* has examined the susceptibility of gaseous

* 'Thèses présentées à la Faculté des Sciences de Paris pour obtenir le grade de Docteur ès Sciences Physiques,' par M. P. Curie, Paris, 1895.' This memoir is of remarkable interest in many ways.

oxygen at different temperatures, and shown that between the limits of 0° C. and 452° C. the magnetic susceptibility of oxygen (K) per unit of mass is a function of the absolute temperature T , such that

$$10^6 K = 33700/T,$$

and that the value of K (per gram) at 0° C. is, therefore, $123/10^6$. The mass of 1 c.c. of oxygen gas at 0° C. and 760 mm. is 0.0014107 gram, and, reciprocally, the volume of one gram is 708.9 c.c. at 0° C. and 760 mm.

Hence the magnetic susceptibility of gaseous oxygen at 0° C. and 760 mm. per unit of volume (one c.c.) would be $123 \times 0.00141 \times 10^{-6} = 0.173 \times 10^{-6}$, which is not very different from that obtained by other observers.*

If then it could be supposed that gaseous oxygen followed the same law down to -182° C., and taking the gas in a condition when the density is nearly 0.00423, the volume susceptibility (k) at -182° C. would be 1.6×10^{-6} , and hence the permeability (μ), where

$$\mu = 1 + 4\pi k,$$

should be 1.00002.

It is, however, certain that the susceptibility per unit of mass will not continue to increase in accordance with the hyperbolic law, because this would imply that at the absolute zero of temperature the susceptibility would be infinitely great, and hence the above number 1.00002 gives a superior limit for the permeability of the gaseous oxygen at -182° C. lying on the surface of the liquid oxygen.†

The conclusion is that the correction to be applied to the above observed value of μ for the liquid oxygen, viz., 1.00287, to refer it to a vacuum taken as unity, is altogether masked by the unavoidable errors of experiment, and hence, pending further more exact measurements, this may be taken as the value of the constant. We have, however, at the present time, arranged a method which will enable us we hope to determine directly the magnetic susceptibility of liquid

* Faraday, 'Experimental Researches,' vol. 3, p. 502, gives a value for the susceptibility of gaseous oxygen at 60° F., referred to an equal volume of water as unity, which, when reduced to absolute values by taking the magnetic susceptibility of water as 0.79×10^{-6} , gives the value of the susceptibility as 0.143×10^{-6} . Becquerel found a value not very different.

† The critical temperature of oxygen is -118° C. The corresponding absolute temperature is 155°. If we then put $T = 155$, in Curie's formula, $10^6 K = 33700/T$, we get $10^6 K = 217.4$, as his deduced extrapolated value for the susceptibility per unit of mass. Since the density of liquid oxygen, as determined by one of us (J. Dewar) is 1.1375, our value for the susceptibility per unit of mass of the liquid oxygen is $228/1.1375 = 200.7$. These figures show that the hyperbola does not represent the value of the susceptibility per unit of mass below the critical temperature.

oxygen with far greater accuracy. This method consists in observing the mechanical force which acts upon a vacuum bulb or mass of matter of known and very low susceptibility when it is suspended free from gravity in a vessel of liquid oxygen, and in a variable magnetic field. Under these conditions a vacuum bulb of very thin glass would behave like a strongly diamagnetic body, and if the magnetic susceptibility of the vacuum bulb or test mass is k_1 , and that of liquid oxygen is k_2 for equal volumes, then the apparent diamagnetic susceptibility of the mass will be $-(k_2 - k_1)$, and the actual paramagnetic susceptibility of liquid oxygen may be deduced from a knowledge of k_1 and $-(k_2 - k_1)$. By this method we hope to be able to determine whether the permeability of liquid oxygen is a function of the magnetising force. The latest experimental results and measurements made with solutions of iron salts, such as those made recently by Mr. J. S. Townsend,* appear to show that the magnetic permeability of solutions of these iron salts is a constant quantity at least for a range of magnetic forces varying from 1 to 9 C.G.S. units.

The value, viz. 1.00287, as determined by us for the magnetic permeability of liquid oxygen, shows that the magnetic susceptibility (k) per unit of volume is $228/10^6$. It is interesting to compare this value with the value obtained by Mr. Townsend for an aqueous solution of ferric chloride, and which he states can be calculated by the equation

$$10^6 k = 91.6 w - 0.77,$$

where w is the weight of salt in grams per cubic centimetre, and k the magnetic susceptibility. Even in a saturated solution, w cannot exceed 0.6, hence, from the above equation, we find the value of the magnetic susceptibility of a saturated solution of one of the most paramagnetic iron salts, viz., ferric chloride, is $54/10^6$ for magnetic forces between 1 and 9. This agrees fairly well with other determinations of the same constant. On the other hand, the magnetic susceptibility of liquid oxygen for the same volume is $228/10^6$, or more than *four times as great*. The unique position of liquid oxygen in respect of its magnetic susceptibility is thus strikingly shown. It is, however, interesting to note that its permeability lies far below that of certain solid iron alloys generally called non-magnetic.

The 12 per cent. manganese steel of Mr. R. A. Hadfield is usually spoken of as non-magnetic, yet the magnetic permeability of this last substance has been shown to be 1.3 or 1.4.

We have applied the foregoing method also to the determination of the magnetic permeability of liquid air. Since liquid air which

* See 'Phil. Trans.,' A, vol. 187, 1896, "Magnetisation of Liquids," J. S. Townsend, M.A.

has been standing in a vacuum vessel for any length of time has a composition which varies with the time and which may contain as much as 75 or 80 per cent. of oxygen, it was not to be expected that very closely consistent results could be obtained in the case of air. The following figures show, however, the observational results :—

PERMEABILITY OF LIQUID AIR.

Throws of Ballistic Galvanometer. Induction Coils balanced.

Exp. I.	$\left\{ \begin{array}{l} 1.5 \text{ mm. to right} \\ 1.2 \text{ " " " } \\ 1.1 \text{ " " " } \end{array} \right\}$	<p>The transformer in liquid air. Current = 38.0 amperes reversed through primary coils.</p>
Exp. II.	$\left\{ \begin{array}{l} 17.0 \text{ mm. to left} \\ 17.5 \text{ " " " } \end{array} \right\}$	<p>The transformer lifted out of liquid air into cold gaseous air at the same temperature as before. 37.5 amperes reversed.</p>
Exp. III.	$\left\{ \begin{array}{l} 0.3 \text{ mm. to right} \\ 0.3 \text{ " " " } \end{array} \right\}$	<p>The transformer in liquid air. Current = 37 amperes reversed through primary coils.</p>
Exp. IV.	$\left\{ \begin{array}{l} 17.0 \text{ mm. to left} \\ 17.0 \text{ " " " } \\ 17.3 \text{ " " " } \end{array} \right\}$	<p>The transformer lifted out of liquid air into cold gaseous air, and at the same temperature as before. Current = 37 amperes reversed through primary coils.</p>
Later.		
Exp. V.	$\left\{ \begin{array}{l} 2.8 \text{ mm. to left} \\ 2.8 \text{ " " " } \end{array} \right\}$	<p>The transformer in liquid air. Current = 36.7 amperes reversed through primary coils.</p>
Exp. VI.	$\left\{ \begin{array}{l} 18.8 \text{ mm. to left} \\ 19.2 \text{ " " " } \\ 19.4 \text{ " " " } \\ 19.8 \text{ " " " } \end{array} \right\}$	<p>The transformer lifted out of liquid air into cold gaseous air, at the same temperature as before. Current = 37 amperes reversed through primary coils.</p>
Exp. VII.	$\left\{ \begin{array}{l} 3.5 \text{ mm. to left} \\ 3.4 \text{ " " " } \end{array} \right\}$	<p>The transformer in liquid air. Current = 36.7 amperes reversed through primary coils.</p>
Exp. VIII.	$\left\{ \begin{array}{l} 22.0 \text{ mm. to left} \\ 22.0 \text{ " " " } \\ 22.0 \text{ " " " } \end{array} \right\}$	<p>The transformer in liquid air. Primary circuit of balancing coil cut out of circuit and 0.1118 ampere reversed through primary of transformer to standardise the ballistic galvanometer.</p>

The results of these observations, when reduced, show that corresponding to a primary current of 37.5 amperes, or a mean magnetising orce of 164 C.G.S. units, the apparent magnetic permeability of liquid air in terms of gaseous air of the same temperature is 1.00240.

At the time of these observations the liquid air used had probably become almost entirely liquid oxygen by the evaporation of the nitrogen. The figure, however, serves to check approximately that of the liquid oxygen.

In conclusion, we desire to express our thanks to Mr. J. E. Petavel for the assistance he has given to us in the above-work. We hope shortly to be able to make a further contribution to this portion of the investigations on which we are engaged, on the electrical and magnetic constants of liquid oxygen, and which will include a determination of the dielectric constant of liquid oxygen, made with the object of determining the extent to which this substance obeys Maxwell's law connecting magnetic permeability, dielectric constant, and optical refractivity.

November 30, 1896.

ANNIVERSARY MEETING.

Sir JOSEPH LISTER, Bart., F.R.C.S., D.C.L., President, in the Chair.

The Report of the Auditors of the Treasurer's Accounts, on the part of the Society, was presented as follows:—

“The total receipts on the General Account during the past year, including balances carried from the preceding year, amount to £8,928 1s. 3d., and the total receipts on account of Trust Funds, including balances from the preceding year, amount to £5,009 0s. 2d. The total expenditure for the same period amounts to £7,287 12s. 3d. on the General Account (including £300 on loan to the Coral Boring Committee), and £3,347 11s. 7d. on account of Trust Funds, leaving a balance on the General Account of £1,605 9s. 4d. at the bankers (which includes £1304 17s. 3d. on deposit—Dr. Ludwig Mond's gift, £54 10s. Publication Grant Account, and £29 11s. 10d. Water Research Account), and in the hands of the Treasurer a balance of £34 19s. 8d.; leaving also at the bankers a balance on account of Trust Funds of £1,661 8s. 7d.”

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary then read the following Lists :—

Fellows deceased since the last Anniversary (Nov. 30, 1895).

On the Home List.

Chambers, Charles.	Mueller, Baron Ferdinand von K.C.M.G.
Childers, Right Hon. Hugh Cul- ling Eardley, F.R.G.S.	Prestwich, Sir Joseph, D.C.L.
Erichsen, Sir John Eric, Bart., F.R.C.S.	Reynolds, Sir John Russell, Bart., M.D.
Green, Alexander Henry, M.A.	Richards, Sir George Henry, Admiral, K.C.B.
Grove, Right Hon. Sir William Robert, D.C.L.	Richardson, Sir Benjamin Ward, M.D.
Harley, George, M.D.	Sharp, William, M.D.
Hind, John Russell, LL.D.	Trimen, Henry, M.B.
Humphry, Sir George Murray, M.D.	Verdon, Hon. Sir George Frederic, K.C.M.G.
Johnson, Sir George, M.D.	Walker, James Thomas, General, R.E., C.B.
Martin, Henry Newell, M.A.	

On the Foreign List.

Daubrée, Gabriel Auguste.	Kekulé, August.
Fizeau, Hippolyte Louis.	Newton, Hubert Anson.
Gould, Benjamin Apthorp.	

Withdrawn.

Bateman, James, M.A.

Fellows elected since the last Anniversary.

Clarke, Lieut.-Colonel Sir George Sydenham, R.E.	Murray, John, Ph.D.
Collie, J. Norman, Ph.D.	Pearson, Prof. Karl, M.A.
Downing, Arthur Matthew Weld, D.Sc.	Stebbing, Rev. Thomas Roscoe Rede, M.A.
Elgar, Francis, LL.D.	Stewart, Prof. Charles, M.R.C.S.
Gray, Prof. Andrew, M.A.	Temple, Sir Richard, Bart., G.C.S.I.
Hinde, George Jennings, Ph.D.	Wilson, William E.
Miers, Prof. Henry Alexander, M.A.	Woodward, Horace Bolingbroke, F.G.S.
Mott, Frederick Walker, M.D.	Wynne, William Palmer, D.Sc.

On the Foreign List.

Gaudry, Albert.
 Heim, Albert.
 Kohlrausch, Friedrich.
 Langley, Samuel Pierpont.
 Lippmann, Gabriel.

Lie, Sophus.
 Metschnikoff, Elias.
 Mittag-Leffler, Gösta.
 Schiaparelli, Giovanni.

The President then addressed the Society as follows:—

Nineteen Fellows and five Foreign Members have been taken from the Royal Society by death since the last Anniversary Meeting.

The deceased Fellows are—

John Russell Hind, December 23, 1895, aged 73.

The Right Hon. Hugh Culling Eardley Childers, January 29, 1896, aged 69.

General James Thomas Walker, February 16, 1896, aged 69.

Charles Chambers, March, 1896, aged 61.

William Sharp, April 10, 1896, aged 91.

Sir John Russell Reynolds, May 29, 1896, aged 68.

Sir George Johnson, June 3, 1896, aged 78.

Sir Joseph Prestwich, June 23, 1896, aged 84.

The Right Hon. Sir William Robert Grove, August 2, 1896, aged 85.

Alexander Henry Green, August 19, 1896, aged 64.

The Hon. Sir George Frederic Verdon, September 13, 1896, aged 62.

Sir John Eric Erichsen, September 23, 1896, aged 78.

Sir George Murray Humphry, September 24, 1896, aged 76.

Baron Ferdinand von Mueller, October 9, 1896, aged 71.

Henry Trimen, October 18, 1896, aged 53.

George Harley, October 27, 1896, aged 67.

Henry Newell Martin, October 28, 1896, aged 44.

Admiral Sir George Henry Richards, November 14, 1896, aged 76.

Sir Benjamin Ward Richardson, November 21, 1896, aged 68.

The Foreign Members are—

Gabriel Auguste Daubrée, May 29, 1896, aged 82.

August Kekulé, July 13, 1896, aged 66.

Hubert Anson Newton, August 12, 1896, aged 66.

Hippolyte Louis Fizeau, September 18, 1896, aged 77.

Benjamin Apthorp Gould, November 27, 1896, aged 72.

Although biographical notices of nearly all will be found in the 'Proceedings,' there are some to whose labours I may make brief reference to-day.

Sir William Grove presented the rare spectacle of steady and distinguished devotion to science in spite of the claims of an exacting profession. Grove was an eminent lawyer. Called to the bar in 1835, he was for some time kept from active work by ill health; but he subsequently acquired a considerable practice, and becoming a Queen's Counsel in 1853, was for some years the leader of the South Wales Circuit. His practice was mainly in patent cases, and the reputation he obtained in that field led to his being appointed a member of the Royal Commission on the Patent Laws. His work as an advocate was, however, by no means confined to such matters; he was one of the counsel—Serjeant Shee and Dr. Kenealy being the others—who defended the Rugeley poisoner, William Palmer, and he was engaged in many other *causes célèbres*.

The eminent position to which he had risen at the bar led to his appointment in November, 1871, as a Judge of the old Court of Common Pleas, a post which in 1875 was converted by the Judicature Act into that of a Judge of the High Court. This office he held until his retirement in 1887, when he became a member of the Privy Council.

Throughout the greater part of his long and distinguished legal career, Grove's love of science impelled him to devote a large share of his energies to its pursuit. It is remarkable that his first paper, which was communicated to the British Association in 1839, and which also appeared in the '*Comptes Rendus*,' and in Poggendorff's '*Annalen*,' contained a description of the "Grove's cell," which was afterwards used in every physical laboratory in the world. This was succeeded by a long series of memoirs, chiefly on electrical subjects, among which one of the best known is that on the gas battery. In 1842 he delivered, at the London Institution, an address which was, in the following year, developed into the celebrated series of lectures: "On the Correlation of Physical Forces." In these he discussed what we should now call the transformations of energy; and, though Professor Tait, in his "Historical Sketch of the Science of Energy,"* assigns precedence in calling "attention to the generality of such transformations" to Mrs. Somerville, there can be no doubt that Grove was an independent and very advanced thinker on that subject.

For many years Sir William Grove took a very prominent part in the affairs of the Royal Society, and was one of the most active promoters of the reform of its constitution, which took place in 1847. It is largely to his efforts that we owe our present system of electing only a specified number of Fellows in each year. He was also one of the founders of the "Philosophical Club."

He was President of the British Association in 1866, and, in the

* '*Thermodynamics*,' p. 58.

course of his address, observed: "The Kew Observatory, the petted child of the British Association, may possibly become an important national establishment; and, if so, while it will not, I trust, lose its character of a home of untrammelled physical research, it will have superadded some of the functions of the Meteorological Department of the Board of Trade, with a staff of skilful and experienced observers."* Although the British Association long ago handed over the care of its "petted child" to a Committee appointed by the Royal Society, the Society and the Association have lately appointed a joint Committee to urge the Government to supply the funds for converting the Kew Observatory into a "national establishment" similar to the Reichsanstalt at Charlottenburg. We are thus striving to realise to-day the suggestion thrown out, thirty years ago, by Grove.

In Sir Joseph Prestwich we have lost almost the last link that remained which connected geologists of the present day with the founders of the science in the first half of this century. To him we are indebted, not only for the first comprehensive classification of the tertiary beds of this country—to several of which he assigned the names by which they will henceforth be universally known—but, also, for their correlation with the strata of the Paris Basin. To him, also, is due the credit of having been the first to establish the authenticity of the remains of human workmanship found in the drift-deposits of the valley of the Somme, and of thus having laid secure foundations on which arguments as to the extreme antiquity of man upon the earth may be based. In France his name was known and respected as much as in England, and it would be hard to say how much of the advance in geological knowledge during the last sixty years was not due to his unintermitted labours, which extended over the whole of that period.

The earliest scientific investigation of Armand Hippolyte Louis Fizeau was on the use of bromine in photography, and was published in 1841. He will always be remembered as the first who carried out experiments designed to measure the velocity of light produced by a terrestrial source, and travelling through a comparatively small distance near the surface of the earth. These observations, made in 1849, were very difficult; but the value of the method employed is attested by the fact that a quarter of a century afterwards it was adopted by M. Cornu, and that with the improved apparatus employed by him it gave results of the highest accuracy.

A few years afterwards Fizeau performed another classical experiment by which he measured the change in the velocity of light produced by the motion of the medium in which it travels.

* 'Correlation and Continuity.' Fifth Edition, 1867, p. 278.

He also devised an extremely delicate method (based on the interference of light) of determining the coefficients of thermal expansion of small bodies, such as crystals. The instrument he designed has been carefully studied by the Bureau International des Poids et Mesures, with very satisfactory results.

On account of these and other researches, M. Fizeau has, for nearly half a century, occupied a conspicuous position among European physicists. He was awarded the Rumford Medal in 1866, and became a Foreign Member of the Royal Society in 1875.

Our distinguished Foreign Member, Professor Hubert Anson Newton, Senior Professor of Mathematics at the Yale University, New Haven, died at his home in New Haven on the 12th of August last. He was born at Sherbourne, in the State of New York, in 1830; studied at Yale College, where he graduated in 1850, and was called to the Chair of Mathematics in the University at the early age of twenty-five.

On the organisation of the Observatory of the University in 1882, Professor Newton was appointed Director; and though he resigned this position in 1884, the whole policy and success of the Observatory ever since, and, indeed, its very existence, are in no small measure due to his warm interest and untiring efforts.

Professor Newton's name will ever remain associated with his important researches on Meteor Astronomy, beginning as early as 1860, and with his inquiry into the possible capture of comets by Jupiter and other planets. His historical investigations, and discussions of the original accounts, showed that the phenomena of meteor showers are of a permanent character, and come within the range of Celestial Dynamics, and that predictions of returning meteoric displays are possible.

Professor Newton was President of the American Association for the Advancement of Science in 1885, and was for many years an Associate Editor of the 'American Journal of Science.' He was a man of noble character, held in universal esteem, and greatly beloved by all those to whom he was personally known.

The death of August Kekulé will be felt as a severe loss to chemical science all over the world. Not only did his great activity in original research enrich organic chemistry with many new and interesting compounds, but his announcement of the tetradic valency of carbon, and, especially, his theoretical conception of the benzene ring, gave an impulse to the study of structural chemistry which has introduced order into the vast array of organic compounds, both of the alcoholic and aromatic types, and has not, even yet, expended itself. In recognition of his life-long work, the Council of the Royal Society awarded Professor Kekulé the Copley Medal in 1885.

Another Foreign Member who has passed away from us during

the year is the distinguished mineralogist and geologist, M. Daubrée. After leaving the École Polytechnique in 1832, he was sent on a mission to investigate the modes of occurrence of tin-ore in Cornwall and on the Continent. His reports showed such ability that he was appointed Professor of Mineralogy and Geology at Strasburg, at the age of 25; afterwards (1861–2) he became Professor of Geology at the Musée d'Histoire Naturelle at Paris, and at the same time Professor of Mineralogy at the École des Mines; in the same year he succeeded to the Chair at the Institut vacated by M. Cordier. From 1872 to 1884, when the rules of the Service made retirement by reason of age compulsory, he acted as Director of the École des Mines. M. Daubrée was the leader in France in experiments for the synthetic reproduction of minerals and rocks, and his laboratory furnace was the first to yield crystals of oxide of tin having the lustre, colour, and hardness of the mineral cassiterite; his memoir on the zeolites and other minerals, produced since Roman times through the action of the hot springs of Plombières on the bricks and concrete, has been of general interest both to mineralogists and geologists. Other important experiments led him to infer that circulating water, rather than heat or vapours, has been the essential agent in all phenomena of rock transformation. M. Daubrée gave much attention to the description and classification of meteorites, and made numerous experiments relative to the reproduction of material having similar characters.

The Council was much occupied during the earlier part of the session with the consideration of the proposed "Standing Orders" relating to the conduct of the meetings, and to the Publications of the Society—a subject which has engaged the anxious attention of previous Councils. In framing these Standing Orders two principal objects were kept in view. Firstly, to increase the interest of the meetings by giving greater freedom in the conduct of them, and by enlarging the opportunities for discussion; and secondly, to obtain a more secure, and, at the same time, more rapid judgment as to the value of communications made to the Society; so that, while the high standard of the 'Philosophical Transactions' is retained, or even raised, greater rapidity in the publication of these and of the 'Proceedings' may be attained. To secure these latter objects, the Council has called to its aid, in the form of Sectional Committees, a number of Fellows much greater than that of the Council itself, to whom will be entrusted the task of reviewing the communications to the Society, and of making to the Council such recommendations with respect to them as may seem desirable. It is further probable that by using the special knowledge of the several Sectional Committees in the detailed consideration of special questions, the Council will have more time at its disposal than it has at present

to consider the matters of larger policy which are so frequently brought before it.

It soon became evident that no satisfactory Standing Orders securing these advantages could be drawn up which would not be in some way or other inconsistent with the Statutes at present in operation. It was accordingly resolved to modify the Statutes; and this has been done by giving to certain Statutes a more general form than that in which they have for a long time appeared, so that such alterations of detail as may from time to time seem desirable may be effected by changes in the Standing Orders only, without interfering with the Statutes. I gladly avail myself of this opportunity of acknowledging the great help which the Council received from Mr. A. B. Kempe, in respect to the many legal points which arose in connection with the change of Statutes. A copy of the Statutes, as amended during the present session, as well as of the Standing Orders adopted, will be found in the Year-book, which has been instituted by one of the new Standing Orders, and which will be published each year, as soon after the Anniversary Meeting as possible.

The International Conference called to consider the desirability and possibility of compiling and publishing, by international co-operation, a Complete Catalogue of Scientific Literature, was duly held; and the Society may be congratulated on the successful issue of a meeting, to the preparations for which a special International Catalogue Committee, appointed by, and acting under the authority of, the Council, had devoted much time and labour. The Conference met in the apartments of the Society on July 14, 15, 16, and 17, under the presidency of the Right Hon. Sir J. Gorst, Vice-President of the Committee of Council on Education, and was attended by forty-one delegates, representing nearly all countries interested in science. The Society was represented by the Senior Secretary, Professor Armstrong (Chairman of the International Catalogue Committee), Mr. Norman Lockyer, Dr. L. Mond, and Professor Rücker. Four other Fellows of the Society, General Strachey, Dr. D. Gill, Professor Liversidge, and Mr. R. Trimen were among the delegates appointed by the Indian and Colonial Governments.

The Conference resolved that it was desirable to compile and publish a catalogue of the nature suggested in the original circular issued by the Royal Society, the administration being carried out by a Central International Bureau, under the direction of an International Council, with an arrangement that each of such countries as were willing to do so, should, by some national organisation, collect and prepare for the Central Bureau all the entries belonging to the scientific literature of the country. It was further resolved that the language of the catalogue should be English, and a proposal that the Central Bureau should be placed in London was carried by

acclamation. The Conference finding itself unable to accept any of the systems of classification proposed, requested the Royal Society to form a committee which should consider this and other matters which were left undecided by the Conference. The Council are already taking steps to perform the duties thus entrusted to them by the Conference.

The delegates of the Society reported that the whole proceedings of the Conference were carried on with remarkable good feeling, and even unanimity, and that the confidence felt and expressed by the various delegates in the fitness of the Royal Society to complete the work begun by the Conference was most gratifying.

In connection with the fact that the proposed International Catalogue is to be in part arranged according to subject matter, it may be stated that the Council, acting upon a resolution of the International Catalogue Committee, have taken steps towards the practice of appending subject indices to the papers published by the Society, and have recommended the same practice to other Societies.

The work connected with the Society's own Catalogue is progressing. Vol. XI, the last of the decade 1874-83, has been published, and the preparation of the Supplement, which has been found necessary for this and preceding decades, is being pushed on.

For the Subject Index to the Catalogue, slips have been prepared, and the Catalogue Committee will soon have to advise the Council as to the system of classification to be adopted.

The Grant of £1000 in aid of publications, which My Lords of the Treasury promised last summer to place upon the Estimates of this year, has been sanctioned by Parliament, and a moiety of it has already been paid to the Society. The Council have already felt the great advantage of having this money at their disposal, and have framed Regulations for its administration which they trust will be found to work satisfactorily.

The Council have made some small changes (which have been approved by My Lords of the Treasury) in the Regulations for the administration of the Government Grant of £4000 in aid of Scientific Inquiries, directed chiefly towards more effectually securing that Grants made should be expended for the purpose for which they were given, and that objects of permanent interest obtained by Grants should be properly disposed of. The only two Grants made this year which call for special mention are that of £1000 to the Joint Permanent Eclipse Committee of the Royal and Royal Astronomical Societies, for observations of the Solar Eclipse of August, and that of £800 for boring a coral reef in the Pacific Ocean, administered by the Committee appointed by the Royal Society, both drawn from the Reserve Fund.

The Expedition to bore the Coral Reef received valuable assistance

from My Lords of the Admiralty, who directed H.M.S. "Penguin" to carry the observers from Sydney, N.S.W., to Funafuti, the seat of the boring, and to render the Expedition all possible help during the whole of the operations. I desire to express on behalf of the Society our recognition of this renewed token of the willingness of My Lords of the Admiralty to further scientific inquiry. Though the full Report of the Expedition has not yet reached the Council, information has been received to the effect that the boring operations had to be suspended when a depth of only 75 feet had been reached; a layer of sand and boulders presenting obstacles which the experts employed were unable to overcome. It is much to be regretted that an undertaking which promised scientific results of very great value has thus so far failed.

The appeals of the Council to H.M. Minister for Foreign Affairs and to My Lords of the Admiralty for assistance to the Eclipse Expeditions met with most cordial and effective response, for which we would express our gratitude. We also desire to acknowledge the courtesy shown and help afforded to the observing parties in Norway and Japan by the respective Governments of those countries, and to record our high appreciation of the enthusiastic and effective aid given to those under the direction of Mr. Norman Lockyer, at Vadsö, by Captain King Hall and the Officers and crew of H.M.S. "Volage"; to Dr. Common, also in Norway, by Commodore Atkinson, of H.M.S. "Active"; to the Astronomer Royal's party, in Japan, by the Officers of H.M.S. "Humber," "Pique," and "Linnet," kindly detached by Admiral Sir A. Buller to convey the various members of the expedition to and from Yezo, and to aid them during the observations.

Both in Norway and in Japan unfavourable weather rendered to a large extent nugatory the elaborate preparations which had been made for observing the eclipse. But British astronomy was splendidly saved from failure on this important occasion by the munificence and public spirit of Sir George Baden Powell, who fitted up, at his own expense, and accompanied an expedition in his yacht "Otaria" to Novaya Zemlya. The instruments employed were provided by our Fellows, Mr. Lockyer and Mr. Stone, of the Radcliffe Observatory, Oxford; and the observations were entrusted to Mr. Shackleton, one of the computers employed by the Solar Physics Committee. In brilliant weather photographic observations were made, which promise to yield novel results of a highly important character.

At the request of the President of the Board of Trade the Council nominated, in March, Professors Kennedy and Roberts-Austen as two members of a Committee to investigate the loss of strength in steel rails. So far as I am aware, the Committee has not yet made

its report. More recently, in July, the Council, at the request of H.M. Secretary for Colonial Affairs, appointed a Committee to consider, and if necessary to investigate, in conjunction with Surgeon-Major Bruce, who has made important researches in the matter, the disease caused in cattle in Africa by the Tsetse Fly. The Committee is still engaged on the inquiry.

We believe that the Council, in cordially responding to requests like the above, and in freely placing at the disposal of H.M. Government its scientific knowledge and its acquaintance with scientific men, is performing one of its most important functions. The Council of the Royal Society is again and again called upon to approach H.M. Government on behalf of the interests of science, and when it does so always meets with a cordial reception and a respectful hearing, even on occasions when public necessities prevent a favourable reply being given to its requests. In return, the Council believes it to be its duty (when called upon to do so), not only to place its own time and labour ungrudgingly at the service of H.M. Government, but also to ask for the co-operation of other Fellows of the Society, or even other scientific men not Fellows of the Society, feeling confident that whenever the matter in hand has practical bearings beyond the simple advancement of Natural Knowledge, the value of a scientific man's time and energy will be duly considered.

Some correspondence has taken place with the War Office relative to resuming the borings in the Delta of the Nile, which were carried on for a time some years ago, and which, though not completed, yielded valuable results. The Expedition to the Soudan has, however, prevented anything being done. The Council learn with pleasure that the old borings, undertaken for a purely scientific object, have indirectly been a valuable means of supplying certain districts of the Delta with sweet water.

If anything had been needed to justify the meetings for discussion recently established, it would have been supplied by the brilliant success of that held during the present session on Colour Photography. On that occasion, M. Lippmann gave us a demonstration of results of unprecedented beauty, obtained by extremely simple means, though based on profound mathematical reasoning. Such meetings can only prove fruitful when they are held in consequence of some theme needing such a discussion as is afforded by a special meeting; and their occurrence must therefore be uncertain and irregular. The purpose for which they were instituted would be frustrated if they were held at times fixed in any formal way, irrespective of whether they were needed or no.

Three of the informal gatherings recently instituted, limited to Fellows of the Society, have been held during the session, and were judged to be very successful.

The Council has had occasion during the past session to present an address of condolence to Her Majesty, the Patron of the Society, on the lamented death of Prince Henry of Battenberg, and to the Royal Academy on the occasion of the death of their President, Lord Leighton. In the absence of Council, during the recess, I sent another message of sympathy on the death of Sir J. Millais.

I had the privilege of presenting on behalf of the Council, an address of congratulation to our late President, Lord Kelvin, on the occasion of his Jubilee, nobly celebrated in Glasgow last summer, by a very remarkable concourse of scientific men from all parts of the world, assembled to do him honour.

Addresses were also sent to our Foreign Member, Professor Cannizzaro, on the celebration of his seventieth birthday, and to the University of Princeton, New Jersey, U.S.A., on the occasion of its Sesquicentenary Anniversary.

Under the guidance of the Scientific Relief Committee, the Council has during the year granted £100 to assist scientific persons or their relatives in distress. The Council desires to call the attention of the Fellows to the fact that, during the year, as during past years, the income of the fund has exceeded its expenditure, and that more aid could be given than has been given. With the view of increasing the usefulness of the fund, the Council has added to the list of those who can make representations to the Council concerning relief the Presidents of the Mathematical, Physical, and Entomological Societies.

I cannot but give expression to my deep regret, shared, I am sure, by every Fellow, that Lord Rayleigh, whose tenure of office as Secretary has been marked as much by faithful devotion to the interests of the Society as by scientific brilliancy, has thought it right, in consequence of increasing pressure of other engagements, to retire. But I rejoice that the Council can submit to your suffrages a man well qualified to wear the mantle laid down by Lord Rayleigh.

The Fellows will be pleased to learn that Mr. Rix, who was compelled by the condition of his health a year ago, to resign the position which he had held for many years with such great advantage to the Society, has much improved under the lighter labour of the Clerkship to the Government Grant Committee.

As his successor in the office of Assistant-Secretary, the Council, out of eighty-four candidates, unanimously selected Mr. Robert Harrison, who entered upon his duties on the 24th of April last.

The scientific work of the Society during the past year has been full of deep and varied interest. Early in the session the announcement of Röntgen's great discovery burst upon the world. Its wonderful applications to medicine and surgery attracted universal attention to it; and physicists everywhere have since been engaged

in investigating the nature of the new rays. Perhaps no outcome of such inquiries has been more remarkable than the fact observed by our Fellow Professor J. J. Thomson, that the rays have the power of discharging electricity, both positive and negative, from a body surrounded by a non-conductor; a mass of paraffin wax, for example, behaving in their path for the time being like a conductor of electricity.

It appears that Lenard had before observed the discharge of both kinds of electricity through air by the rays with which he worked. Lenard's rays, however, differ from Röntgen's in being deflectable by a magnet, implying, in the opinion of most British physicists, that they are emanations of highly electrified particles of ponderable matter, while Röntgen's are regarded as vibrations in the ether. The question naturally arises whether Lenard, in the observations referred to, may not have been working with a mixture of Röntgen's rays and his own. While points like these are still under discussion by experts, we cannot but feel that the letter X, the symbol of an unknown quantity, employed originally by Röntgen to designate his rays, is still not inappropriate.

I have before referred to Lippmann's beautiful demonstration and discussion of colour photography in one of our meetings.

Very important researches have been made both by Lord Rayleigh and by Professor Ramsay into the physical properties of the new substance, helium, discovered by Ramsay in the previous session. Among their most striking results is the fact ascertained by Rayleigh that the refractivity of helium is very much less than any previously known, being only 0.146; between three and four times less than that of hydrogen, the lowest that had before been observed, although helium has more than twice the density of hydrogen. And equally surprising is Ramsay's observation of the extraordinary distance through which electric sparks will strike through helium, viz., 250 or 300 mm. at atmospheric pressure, as compared with 23 mm. for oxygen and 39 for hydrogen. Such properties appear to indicate that in helium we have to do with an exceedingly remarkable substance.

The density of helium appears to be really slightly different according to the mineral source from which it is obtained; and this circumstance seems to give countenance to the opinion arrived at by Lockyer and also by Runge and Paschen, from spectroscopic investigation, that helium is not a perfectly pure gas. But whatever other gas or gases may be mixed with it, they must be as inert chemically as the main constituent; for all Ramsay's elaborate attempts to induce it, or any part of it, to combine with other bodies have entirely failed.

Professor Roberts-Austen, in the Bakerian lecture, brought before

us astonishing evidence that metals are capable of diffusing into each other, not only when one of them is in the state of fusion, but when both are solid. We learned that if clean surfaces of lead and gold are held together *in vacuo* at a temperature of only 40° for four days, they will unite firmly and can only be separated by a force equal to one-third of the breaking strain of lead itself. And gold placed at the bottom of a cylinder of lead 70 mm. long thus united with it, will have diffused to the top in notable quantities at the end of three days. Such facts tend to modify our views concerning the mutual relations of the liquid and solid states of matter.

Such are a few samples of the many highly interesting communications we have had in physics and chemistry. On the biological side also, there has been no lack of important work. Of this I may refer to one or two instances.

Professor Schäfer has given us an account of the well devised experiments by which he has conclusively established that the spleen is on the one hand capable, like the heart, of independent rhythmical contractions, and, on the other hand, has those contractions controlled by the central nervous system acting through an extraordinary number of efferent channels.

Professor Farmer and Mr. Lloyd-Williams made a very beautiful contribution to biology in the account they gave of their elaborate investigations on the fertilisation and segmentation of the spore in *Fucus*. Especial interest attached to this communication, from the fact that it described in a vegetable form exactly what had been established by Oscar Hertwig in *Echinodermata*, viz., that out of the multitude of fertilising elements that surround the female cell, one only enters it and becomes blended with its nucleus.

Lastly, I may mention the very remarkable investigation into the development of the Common Eel, which was described to us a fortnight ago by Professor Grassi, to which I shall have occasion to refer in some detail when speaking of his claims to one of the Society's medals.

These, as I have before said, are but samples of what we have had before us; but I think they are in themselves sufficient to justify the statement that, in point of scientific interest, the past year has been in no degree inferior to its predecessors.

COPLEY MEDAL.

Professor Carl Gegenbaur, For. Mem. R.S.

The Copley Medal for 1896 is given to Carl Gegenbaur, Professor of Anatomy in Heidelberg, in recognition of his pre-eminence in the science of Comparative Anatomy or Animal Morphology. Professor

Gegenbaur was born in 1826, and a few weeks ago his 70th birthday was celebrated by his pupils (who comprise almost all the leading comparative anatomists of Germany, Holland, and Scandinavia) by the presentation to him of a "Festschrift" in three volumes. Gegenbaur is everywhere recognised as the anatomist who has laid the foundations of modern comparative anatomy on the lines of the theory of descent, and has to a very large extent raised the building by his own work. His 'Grundzüge der vergleichenden Anatomie' was first published in 1859, when he was 33 years old. In the second edition, published in 1870, he remodelled the whole work, making the theory of descent the guiding principle of his treatment of the subject. Since then he has produced a somewhat condensed edition of the same work under the title of 'Grundriss' (translated into English and French), and now, in his 71st year, he is about to publish what will probably be the last edition of this masterly treatise, revising the whole mass of facts and speculations accumulated through his own unceasing industry and the researches of his numerous pupils during the past quarter of a century.

Gegenbaur may be considered as occupying a position in morphology parallel to that occupied by Ludwig in Physiology. Both were pupils of Johannes Müller, and have provided Europe with a body of teachers and investigators, carrying forward in a third generation the methods and aims of the great Berlin professor. Gegenbaur's first independent contribution to science was published in 1853. It was the outcome of a sojourn at Messina in 1852, in company with two other pupils of Johannes Müller, namely Albert Kölliker (still professor in Würzburg) and Heinrich Müller, who died not long afterwards. These young morphologists published the results of their researches in common. Gegenbaur wrote on Medusæ, on the development of Echinoderms, and on Pteropod larvæ. A long list of papers on the structure and development of Hydrozoa, Mollusca, and various invertebrata followed this first publication. The greatest interest, however, was excited among anatomists by his researches on the vertebrate skeleton (commenced already in 1849 with a research, in common with Friedreich, on the skull of axolotl). In a series of beautifully illustrated memoirs he dealt with and added immensely to our knowledge of the vertebral column, the skull, and the limb-girdles and limbs of Vertebrata, basing his theoretical views as to the gradual evolution of these structures in the ascending series of vertebrate forms upon the study of the cartilaginous skeleton of Elasmobranch fishes, and on the embryological characters of the cartilaginous skeleton and its gradual replacement by bone in higher forms. His method and point of view were essentially similar to those of Huxley, who independently and contemporaneously was engaged on the same line of work.

For many years Gegenbaur was professor in Jena, where he was the close friend and associate of Ernst Haeckel, but in 1875 he accepted the invitation to the Chair of Anatomy in Heidelberg, and in view of the increased importance of his duties as a teacher of medical students, and therefore of human anatomy, though still continuing his researches on vertebrate morphology, he produced a large treatise on that subject, which has run through two editions. In this work he made the first attempt to bring, as far as possible, the nomenclature and treatment of human anatomy into thorough agreement with that of comparative anatomy, and to a very large extent the changes introduced by him have influenced the teaching of human anatomy throughout Europe and America.

There is probably no comparative anatomist or embryologist in any responsible position at the present day who would not agree in assigning to Gegenbaur the very first place in his science as the greatest master and teacher who is still living amongst us. He is not only watching in his old age the developments of his own early teachings and the successful labours of his very numerous disciples, but is still exhibiting his own extraordinary industry in research, his keenness of intellectual vision, and his unrivalled knowledge and critical judgment.

ROYAL MEDAL.

Sir Archibald Geikie, F.R.S.

One of the Royal Medals is conferred on Sir Archibald Geikie, on the ground that of all British geologists he is the most distinguished, not only as regards the number and the importance of the geological papers which he has published as an original investigator, but as one whose educational works on geology have had a most material influence upon the advancement of scientific knowledge.

His original papers range over many of the main branches of geological science. His memoir upon the 'Glacial Drift of Scotland' (1863) is one of the classics in British geology. His work on the 'Scenery of Scotland, viewed in connection with the Physical Geology' (1865) was the first successful attempt made to explain the scenery of that country upon scientific principles, and is still without a rival. His papers on the "Old Red Sandstone of Western Europe" (1878-79) gave for the first time a clear and convincing picture of the great lake period of British geology, founded upon personal observation in the field.

His many original contributions to the Volcanic History of the British Isles form a succession of connected papers, crowded with important observations and discoveries, and brilliant and fertile generalizations respecting the abundant relics of former volcanic

activity in the British Isles from the earliest geological ages to Middle Tertiary times.

In the first series of these papers—commencing with the “Chronology of the Trap Rocks of Scotland” (1861), and ending with the “Tertiary Volcanic Rocks of the British Isles” (1869), abundant original proofs were advanced of the activity of volcanic action in the Western Isles of Scotland, and of its long duration in geological time. The second series (1871–88) was especially distinguished by the publication of his remarkable paper on the “Carboniferous Volcanic Rocks in the Basin of the Firth of Forth,” our earliest, and, as yet, our only monograph on a British volcanic area belonging to a pre-Tertiary geological system. The third series (begun in 1888) commenced with his memoir on the “History of Volcanic Action during the Tertiary Period in the British Isles,” a paper which is by far the most detailed and masterly contribution yet made to the subject, and for which the Brisbane Medal was awarded him by the Royal Society of Edinburgh; and this succession of papers has been followed by the publication of others of almost equal importance.

Sir Archibald Geikie has also written many papers and memoirs bearing upon geological processes and their effects, which have become permanent parts of our scientific literature.

While carrying out this highly important original work in Geology, Sir Archibald has most materially contributed to the advancement and diffusion of scientific knowledge by his many educational works upon Geology and Physical Geography. His ‘Elementary Lessons on Physical Geography’ has passed through several English and Foreign editions; his ‘Outlines of Field Geology’ is now in its fifth edition; and his article on Geology—originally contributed to the ‘Encyclopædia Britannica’ in 1879—was afterwards expanded by him into his well-known ‘Text-book of Geology,’ which has become the acknowledged British standard of Geology in general.

ROYAL MEDAL.

Professor C. V. Boys.

The other Royal Medal is awarded to Professor Boys, who has given to physical research a method of measuring minute forces far exceeding in exactness any hitherto used, by his invention of the mode of drawing quartz fibres, and by his discovery of their remarkable property of perfect elastic recovery.

Professor Boys has himself made several very important researches in which he has employed these fibres to measure small forces. Using a combination of a thermo-junction with a suspended coil in a galvanometer of the usual D’Arsonval type, a combination first devised by D’Arsonval himself, Professor Boys developed the idea in the micro-

radiometer, an instrument rivalling the bolometer in the measurement of small amounts of radiation. Its sensitiveness and accuracy were obtained in part by the use of a quartz fibre to suspend the coil, in part by the admirable design of every portion of the instrument. Professor Boys was the first to show its value in an investigation into the radiation received from the moon and stars.

In his great research on the value of the Newtonian constant of attraction, Professor Boys used quartz fibres to measure the gravitation forces between small bodies by the Michell-Cavendish torsion method. He redesigned the whole of the apparatus, and, calculating what should be the dimensions and arrangements to give the best results, he was led to the remarkable conclusion that accuracy was to be gained by a very great reduction in the size of the apparatus. This conclusion he justified by a determination of the value of the Newtonian constant, which is now accepted as the standard.

Professor Boys has also made some remarkable studies by a photographic method of the motion of projectiles, and of the air through which they pass.

All his work is characterised by the admirable adjustment of the different parts of the apparatus he uses to give the best results. His instruments, are, indeed, models of beauty of design.

RUMFORD MEDAL.

Professor Philip P. Lenard and Professor W. C. Röntgen.

In the case of the Rumford Medal, the Council have adopted a course, for which there are precedents in the awards of the Davy Medal, but which is, as far as the Rumford Medal itself is concerned, a new departure. They have decided to award the Medal in duplicate. It has often happened in the history of science that the same discovery has been made almost simultaneously and quite independently by two observers, but the joint recipients of the Rumford Medal do not stand in this relation to each other. Each of them may fairly claim that his work has special merits and characteristics of its own. To day, however, we have to deal, not with points of difference, but with points of similarity. There can be no question that a great addition has recently been made to our knowledge of the phenomena which occur outside a highly exhausted tube through which an electrical discharge is passing.

Many physicists have studied the luminous and other effects which take place within the tube; but the extension of the field of inquiry to the external space around it is novel and most important. There can be no doubt that this extension is chiefly due to two men—Professor Lenard and Professor Röntgen.

The discussion which took place at the recent meeting of the British Association at Liverpool proved that experts still differ as to the exact meaning and causes of the facts these gentlemen have discovered. No one, I believe, disputes the theoretical interest which attaches to the researches of both; or the practical benefits which the Röntgen rays may confer upon mankind as aids to medical and surgical diagnosis. But whatever the final verdict upon such points may be, the two investigators whom we honour to-day have been toilers in a common field, they have both reaped a rich harvest, and it is, therefore, fitting that the Royal Society should bestow upon both of them the Medal which testifies to its appreciation of their work.

DAVY MEDAL.

Professor Henri Moissan.

The Davy medal is given to Professor Henri Moissan.

Notwithstanding the abundant occurrence of fluorine in nature, the chemical history of this element and its compounds has until recently been scanty in the extreme, and, as far as the element in the free state is concerned, an entire blank. And yet from its peculiar position in the system of elements, the acquisition of a more extended knowledge of its chemical properties has always been a desideratum of the greatest scientific interest.

The frequent attempts which have been made from time to time to clear up its chemical history have been constantly baffled by the extraordinary difficulties with which the investigation of this element is beset.

Thanks to the arduous and continuous labours of M. Moissan, this void has been filled up. He has effected the isolation of fluorine in a state of purity, and prepared new and important compounds, the study of which has placed our knowledge of the chemical and physical properties of this element on a level with that of its immediate allies.

During the last few years M. Moissan has turned his attention to the study of chemical energy at extremely high temperatures, and by the aid of the electric furnace, which he has contrived, he has succeeded in obtaining a large number of substances whose very existence was hitherto undreamt of. It is impossible to set bounds to the new field of research which has thus been opened out. The electric furnace of M. Moissan has now become the most powerful synthetical and analytical engine in the laboratory of the chemist.

On studying the accounts which Moissan has given of his researches, we cannot fail to be struck with the originality, care, perseverance and fertility of resource with which they have been carried

on. The Davy Medal is awarded to him in recognition of his great merits and achievements as an investigator.

DARWIN MEDAL.

Professor Giovanni Battista Grassi.

The Darwin Medal for 1896 is awarded to Professor Grassi, of Rome (late of Catania), for his researches on the constitution of the colonies of the Termites, or White Ants, and for his discoveries in regard to the normal development of the Congers, *Murænæ*, and Common Eels from *Leptocephalus larvæ*.

From a detailed examination of the nature and origin of the colonies of the two species of Termites which occur in the neighbourhood of Catania, viz., *Termes lucifugus* and *Callotermes flavicollis*, he was able to determine certain important facts which have a fundamental value in the explanation of the origin of these and similar polymorphic colonies of insects, and are of first-rate significance in the consideration of the question of the share which heredity plays in the development of the remarkable instincts of "neuters," or arrested males and females, in these colonies. Professor Grassi has, in fact, shown that the food which is administered by the members of a colony to the young larvæ determines, at more than one stage of their development, their transformation into kings or queens, or soldiers or workers as the case may be, and the value of these researches is increased by the observations which he has made on the instincts of the different forms, showing that they do not in early life differ from one another in this respect, and are all equally endowed with the potentiality of the same instincts. These do not, however, all become developed and cultivated in all alike, but become specialised, as does the physical structure in the full-grown forms.

A very different piece of work, but having a no less important bearing on the theory of organic evolution, is that on the *Leptocephali*. These strange, colourless, transparent, thin-bodied creatures, with blood destitute of red corpuscles, had been regarded as a special family of fishes, but have been proved by Grassi's patient and long-continued labours to be larval forms of the various *Murænoids*. The most astonishing case is that of the Common Eel, *Anguilla vulgaris*, the development of which had been a mystery since the days of Aristotle. It had been long known that large eels pass from rivers into the sea at certain seasons, and that diminutive young eels, called in this country Elvers, ascend the rivers in enormous numbers. But, although the species is very widely distributed, no one in any country had been able to discover how the elvers were produced. Grassi has shown that, large as the eels are that pass into the sea, they are not perfectly developed fish, but only attain maturity in the depths of the

ocean. There they in due time breed, and from their eggs are hatched the young *Leptocephali*, which, after attaining a certain size, cease to feed, and assume the very different form of the elver. The possibility of establishing these remarkable facts depended on the powerful oceanic currents that prevail about the Straits of Messina, bringing up occasionally to the surface the inhabitants of the depths of the sea. Grassi was thus able to obtain from time to time both adult eels with fully developed sexual organs and their larval progeny, and he actually observed in an aquarium the development of a *Leptocephalus brevirostris* into an elver.

Such highly meritorious contributions to evolution are fitly recognised by the award of the Darwin Medal.

The Statutes relating to the election of Council and Officers were then read, and Professor Liversidge and Dr. Common having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—Sir Joseph Lister, Bart., F.R.C.S., D.C.L.

Treasurer.—Sir John Evans, K.C.B., D.C.L., LL.D.

Secretaries.— { Professor Michael Foster, M.A., M.D., D.C.L., LL.D.
 { Professor Arthur William Rücker, M.A., D.Sc.

Foreign Secretary.—Edward Frankland, D.C.L., LL.D.

Other Members of the Council.

Prof. William Grylls Adams, M.A.; Professor Thomas Clifford Allbutt, M.D.; Professor Robert Bellamy Clifton, M.A.; William Turner Thiselton Dyer, C.M.G.; Prof. James Alfred Ewing, M.A.; Lazarus Fletcher, M.A.; Walter Holbrook Gaskell, M.D.; Prof. Alfred George Greenhill, M.A.; William Huggins, D.C.L.; Prof. Charles Lapworth, LL.D.; Major Percy Alexander MacMahon, R.A.; Prof. Raphael Meldola, F.C.S.; Prof. William Ramsay, Ph.D.; The Lord Walsingham, M.A.; Prof. Walter Frank Raphael Weldon, M.A.; Adml. William James Lloyd Wharton, C.B.

The thanks of the Society were given to the Scrutators.

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55A. 2R. 2P.), rent £77 per annum.

Ground Rent of House, No. 57, Basinghall Street, rent £380 per annum.

" of 23 houses in Wharton Road, West Kensington, rents £253 per annum.

Fee Farm Rent, near Lewes, Sussex, £19 4s. per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, about £52 per annum, Croonian Lecture Fund.

Stevenson Bequest. Chancery Dividend. One-fourth annual interest on balance of Bequest still in Court. (This year, £89 0s. 1d.)

£15,200 Mortgage Loan, 3½ per Cent., to the Duke of Norfolk.

{ being £11,196 10s. 8d. on account of the following Funds:—

Rumford Fund	£	s.	d.
Wintringham Fund	2,367	2	6
Gassiot Trust	1,200	0	0
Sir J. Copley Fund	500	0	0
Jodrell Fund	1,666	13	4
Brady Library Fund	5,182	14	10
	280	0	0

{ and £3,518 0s. 3d. in Chancery, arising from sale of the Coleman Street Estate.—General Purposes.

£14,714 10s. 11d., 2½ per Cent. Consolidated Stock

{ £800 0s. 0d. Scientific Relief Fund.

{ £403 9s. 8d. Bakerian and Copley Medal Fund.

{ £47 19s. 2d. Joule Memorial Fund.

£1,251 8s. 10d. 2½ per Cent. Annuities

£3,000 India 3½ per Cent. Stock.—General Purposes

£1,300 India 3 per Cent. Stock.—General Purposes (Earl of Derby's Bequest).

£800 Midland Railway 3 per Cent. Debenture Stock.—Keck Bequest.

£370 3s. 7d. Midland Railway 4 per Cent. Perpetual Guaranteed Preference Stock.—General Purposes (Stevenson Bequest).

£5,660 Madras Railway Guaranteed 5 per Cent. Stock { General Purposes, £5,000.

£10,000 Italian Irrigation (Cavour Canal) Bonds.—The Gassiot Trust.

£9,061 6s. 8d. Great Northern Railway 3 per Cent. Debenture Stock { Scientific Relief Fund, £7,200.

{ The Trevelyan Bequest, £1,861 6s. 8d.

£5,080	Great Northern Railway Perpetual 4 per Cent. Guaranteed Stock.—Donation Fund.
£2,725	" " " 4 per Cent. Perpetual Preference Stock.—General Purposes (Stevenson Bequest).
£5,000	Metropolitan 3½ per Cent. Stock.—Fee Reduction Fund.
£258 9s. 2d.	Metropolitan 3 per Cent. Stock.—Buchanan Medal Fund.
£9,333	London and North Western Railway 3 per Cent. Perpetual Debenture Stock.—Fee Reduction Fund.
£20,908	" " " 4 per Cent. Consolidated Guaranteed Stock.—{ £6,000 Scientific Relief Fund. £12,150 General Purposes. £2,758 " " (Stevenson Bequest).
£5,000	" " " Consolidated 4 per Cent. Preference Stock.—General Purposes.
£5,000	North Eastern Railway 4 per Cent. Preference Stock.—General Purposes.
£2,760	" " " Consolidated 4 per Cent. Guaranteed Stock.—General Purposes (Stevenson Bequest).
£2,500	South Eastern Railway 4 per Cent. Debenture Stock.—Darwin Medal Fund.
£4,340	South Eastern Railway 5 per Cent. Debenture Stock.—Scientific Relief Fund.
£3,333	London and South Western Railway 4 per Cent. Preference Stock.—General Purposes.
£4,798	Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock.—Handley Fund.
£1,000	London, Brighton, and South Coast Railway Consolidated Guaranteed 5 per Cent. Stock.—Joule Memorial Fund.
£4,000	Southern Maharratta Railway 4 per Cent. Debenture Stock.—General Purposes.
£1,304 17s. 3d.	on Deposit Account at Bank, Mr. Ludwig Mond's Gift.—Catalogue Account.
£1,000	Policy in the Atlas Assurance Office, becoming due October 7th, 1899, No. 24644.—Catalogue Account.
£1,000	Bond.—Dr. Gunning.—Interest to be applied to the promotion of Physics and Biology.
£300	Loan to Coral Reef Boring Committee.

JOHN EVANS, *Treasurer.*

We, the Auditors of the Treasurer's Accounts on the part of the Council, have examined these Accounts and found them correct.

JOSEPH LISTER.
P. A. MACMAHON.
OSBERT SALVIN.

We, the Auditors of the Treasurer's Accounts on the part of the Society have examined these Accounts and found them correct.

SHELFORD BIDWELL.
T. G. BONNEY.
HORACE T. BROWN.

Trust Funds. 1896.

Scientific Relief Fund.

£6,000 L. & N.W.R. 4 per Cent. Consolidated Guaranteed Stock.
 £7,200 Great Northern Railway 3 per Cent. Debenture Stock.
 £4,340 South Eastern Railway 5 per Cent. Debenture Stock.
 £800 2½ per Cent. Annuities.

<i>Dr.</i>		<i>Cr.</i>	
	£ s. d.		£ s. d.
To Balance, Income in hand	925 2 11	By Grants	147 0 0
Less Capital over Invested	36 16 10	„ Purchase of £800 2½ per Cent. Annuities	849 1 0
Dividends	650 11 4	„ Balance, Income in hand	1,450 2 6
Income Tax recovered	21 8 3	Less Capital over Invested	885 17 10
			£1,560 5 8

Donation Fund.

£5,080 Great Northern Railway Perpetual 4 per Cent. Guaranteed Stock.
 The Trevelyan Bequest. £1,861 6s. 8d. Great Northern Railway 3 per Cent. Debenture Stock.

	£ s. d.		£ s. d.
To Balance	482 8 2	By Grants	631 2 11
Dividends	248 9 8	„ Balance	262 3 1
Transfer from Jodrell Fund	142 5 7		
Income Tax recovered	8 2 7		
Grant refunded	12 0 0		
			£893 6 0

Ramford Fund.

£2,367 2s. 6d. 2½ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	49	1	8	By Balance	114	1	4
" Dividends	62	18	4				
" Income Tax recovered	2	1	4				
	£114	1	4		£114	1	4

Bakerian and Copley Medal Fund.

Sir Joseph Copley's Gift. £1,666 13s. 4d. 2½ per Cent. Consolidated Stock.

£403 9s. 8d 2½ per Cent. Annuities.

	£	s.	d.		£	s.	d.
To Balance	108	13	3	By Gold Medal	4	12	1
" Dividends, New 2½ per Cent. Stock	9	15	0	" Bakerian Lecture, Prof. Roberts-Austen	4	0	0
" Dividend—Sir J. Copley's Fund	44	6	4	" Gift:—Prof. C. Weierstrass	50	0	0
" Income Tax recovered	1	15	2	" Balance	105	17	8
	£164	9	9		£164	9	9

The Keck Bequest.

£800 Midland Railway 3 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Dividends	23	4	0	By Balance	23	18	5
" Income Tax recovered	15		2	" Payment to Foreign Secretary	23	18	5
	£23	19	2		£23	19	2

Winttingham Fund.

£1,200 2¼ per Cent. Consolidated Stock.

To Balance ..	£	s.	d.	By Payment to Foundling Hospital	£	s.	d.
" Dividends ..	34	8	8	" Balance ..	34	8	8
" Income Tax recovered ..	31	18	0		32	18	11
	1	0	11				
	£67	7	7		£67	7	7

Croonian Lecture Fund.

One-fifth of the clear rent of an Estate at Lambeth Hill, from the College of Physicians, about £52 per annum.

To Balance	£	s.	d.
" Rent	2	6	0
" Income Tax recovered	50	5	4
	1	12	11
	<hr/>		
	£54	4	3
	<hr/>		

By Lecture (1896), Dr. Waller	£	s.	d.
" Balance	50	0	0
	4	4	3
	<hr/>		
	£54	4	3
	<hr/>		

Davy Medal Fund.

£660 Madras Railway Guaranteed 5 per Cent. Stock.

To Balance	£	s.	d.
" Dividends	50	8	10
" Income Tax recovered	32	1	0
	1	0	11
	<hr/>		
	£83	10	9
	<hr/>		

By Gold Medal	£	s.	d.
" Balance	32	4	6
	51	6	3
	<hr/>		
	£83	10	9
	<hr/>		

The Gussiol Trust.

£10,000 Italian Irrigation Bonds.
£500 2 $\frac{3}{4}$ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	80	18	6	By Payments to Kew Committee	444	11	4
" Dividends	457	17	0	" Law Charges	6	5	2
" Income Tax recovered	14	19	3	" Balance	102	18	3
	£553	14	9		£553	14	9

Handley Fund.

£4,798 Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock.

	£	s.	d.		£	s.	d.
To Dividends	185	10	4	By Transfer to Catalogue Account	191	11	11
" Income Tax recovered	6	1	7		£191	11	11
	£191	11	11				

The Jodrell Fund.

£5,182 14s. 10d. 2 $\frac{3}{4}$ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Dividends	137	15	8	By Transfer to Donation Fund	142	5	7
" Income Tax recovered	4	9	11		£142	5	7
	£142	5	7				

Fee Reduction Fund.

£5,000 Metropolitan 3½ per Cent. Stock.
 £9,333 London and North Western Railway 3 per Cent. Perpetual Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	123	3	2	By Transfer to Royal Society General Account.....	362	0	0
" Dividends	439	6	3	" Purchase of £100 Metropolitan 3½ per Cent. Stock	122	3	6
" Income Tax recovered.....	14	5	11	" Balance	92	11	10
	<hr/>				<hr/>		
	£576	15	4		£576	15	4
	<hr/>				<hr/>		

Darwin Medal Fund.

£2,500 South Eastern Railway 4 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	170	5	2	By Purchase of £100 South Eastern Railway 4 per Cent. Debenture Stock	152	6	6
" Dividends	96	13	4	" Balance	117	10	3
" Income Tax recovered.....	2	18	3				

Joule Memorial Fund.

£1,000 London, Brighton, and South Coast Railway Consolidated Guaranteed 5 per Cent. Stock.
 £47 19s. 2d. 2½ per cent. Annuities.

	£	s.	d.		£	s.	d.
To Balance	82	1	6	By Studentship, J. D. Chorlton	50	0	0
" Dividends, &c.	49	6	6	" Purchase of £47 19s. 2d. 2½ per cent. Annuities...	50	0	0
" Income Tax recovered	1	11	9	" Balance	82	19	9
" Deposit Account withdrawn	50	0	0				
	<hr/>						
	£182	19	9		£182	19	9
	<hr/>				<hr/>		

Brady Library Fund.

£280 2½ per Cent. Consolidated Stock.

To Balance.....	£	s.	d.		£	s.	d.
" Dividends ..	26	2	3		33	14	10
" Income Tax recovered.....	7	9	0	By Balance			
	3	7					
	<hr/>				<hr/>		
	£33	14	10		£33	14	10
	<hr/>				<hr/>		

Gunning Fund.

£1,000 4 per Cent. Bond of His Excellency Dr. Gunning.

To Balance	£	s.	d.		£	s.	d.
" Interest	40	0	0	By Balance	80	0	0
	40	0	0				
	<hr/>				<hr/>		
	£80	0	0		£80	0	0
	<hr/>				<hr/>		

Buchanan Medal Fund.

£258 9s. 2d. Metropolitan 3 per Cent. Stock.

To Balance	£	s.	d.		£	s.	d.
" Dividends ..	9	7	6	By Balance	16	17	6
	7	10	0				
	<hr/>				<hr/>		
	£16	17	6		£16	17	6
	<hr/>				<hr/>		

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Professors Albert Heim, Gabriel Lippmann, G. Mittag-Leffler, and G. Schiaparelli were, at the meeting on the 26th of November, balloted for and elected Foreign Members of the Society.

The following Table shows the progress and present state of the Society with respect to the number of Fellows :—

	Patron and Royal.	Foreign.	Com- pounders.	£4 yearly.	£3 yearly.	Total.
Nov. 30, 1895 ..	4	41	141	112	200	498
Since Elected ..		+ 9	+ 2	+ 1	+ 13	+ 25
Since Compounded			+ 1		— 1	
Since Deceased ..		— 5	— 6	— 10	— 3	— 24
Withdrawn ..				— 1		— 1
Nov. 30, 1896 ..	4	45	138	102	209	498

Account of Grants from the Donation Fund in 1895-96.

£ s. d.

Dr. Gamgee, in aid of his Researches on the Behaviour of Hæmoglobin, &c., toward Ultra-violet Rays.....	50	0	0
Coral Reef Committee, towards the Purchase of Diamonds for Boring a Coral Atoll in the Pacific Ocean	150	0	0
Dr. M. Foster, for Dr. W. Poole, Medical Officer of the British Central African Protectorate, for the Purchase of a Microscope to aid him in his Researches	21	2	11
Sir A. Geikie, in aid of Mr. Reid's Geological Borings at Hoxne	30	0	0
Sir A. Geikie, to assist him in Excavations at Hitchin	50	0	0
Profs. Fleming and Dewar, in aid of their Researches on the Diamagnetic qualities of Metals at Low Temperatures	50	0	0
Prof. Burdon Sanderson, in aid of his Investigations in relation to Tuberculin	60	0	0
Dr. Vaughan Harley, in further aid of his Researches on Absorption from the Alimentary Canal	25	0	0
Dr. J. G. Stoney, for Calculations of the Positions of the November Meteors	15	0	0
Professor Sherrington, to aid him in his Researches on the Nervous System	50	0	0
Marine Biological Association, towards the Purchase of a Steam Yacht for trawling	100	0	0

£601 2 11

December 10, 1896.

Sir JOSEPH LISTER, Bart., F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President announced that he had appointed as Vice-Presidents—

The Treasurer.

Professor Clifton.

Mr. Thiselton Dyer.

Dr. Huggins.

The following Papers were read:—

- I. "On Professor Hermann's Theory of the Capillary Electrometer." By GEORGE J. BURCH, M.A. Communicated by Professor BURDON SANDERSON, F.R.S.
- II. "An Attempt to determine the Adiabatic Relations of Ethyl Oxide." By E. P. PERMAN, D.Sc., W. RAMSAY, Ph.D., F.R.S., and J. ROSE-INNES, M.A., B.Sc.
- III. "The Chemical and Physiological Reactions of certain Synthesised Proteid-like Substances.—Preliminary Communication." By JOHN W. PICKERING, D.Sc. (Lond.). Communicated by Professor HALLIBURTON, F.R.S.
- IV. "An Experimental Examination into the Growth of the Blastoderm of the Chick." By RICHARD ASSHETON, M.A. Communicated by ADAM SEDGWICK, F.R.S.

"On Professor Hermann's Theory of the Capillary Electrometer." By GEORGE J. BURCH, M.A. Communicated by Professor BURDON SANDERSON, F.R.S. Received July 21, —Read December 10, 1896.

I have received, by the courtesy of Professor Hermann, a copy of his paper* on "Das Capillar-Electrometer und die Actionsströme des

* 'Archiv für die Ges. Physiologie,' vol. 63, p. 446.

Muskels," in which he discusses the analyses of certain electrometer curves of muscle variation described by Professor Burdon Sanderson.*

His first statement demands an explanation on my part. He says, "Bevor ich auf Sanderson's Versuche und Schlüsse eingehe, möchte ich zeigen dass der von Burch und von Einthoven aufgestellte, das Capillar-Electrometer betreffende Satz, welcher der Construction zu Grunde liegt, auch aus meiner Theorie des Instruments unmittelbar folgt, was beide Autoren, obwohl sie meine Arbeit erwähnen, nicht bemerkt haben. Da beide ihren Satz *empirisch* gewonnen haben, so kann derselbe als eine schöne Bestätigung meiner Theorie betrachtet werden."

As a matter of fact, I did not know of Professor Hermann's paper until after I had formed my own theory. In my second paper† on the subject I mentioned that it had also been treated by him, "mainly from a mathematical standpoint," and implied that, in my opinion, his data were insufficient. I still think so, and cannot admit that my experimental results prove the correctness of his views.

That a mathematical formula, based upon a certain hypothesis, should agree with observed facts may be strong evidence in its favour, but is not necessarily a proof of the soundness of the hypothesis.

For instance, the equation

$$p = E \cdot e^{-rt}$$

may represent the discharge of a Leyden jar through a circuit of no inductance, or the swing of a pendulum in treacle. That it happens to be also the expression for the time-relations of the capillary electrometer does not of itself imply that the same causes are at work in all three cases, but simply that the forces concerned are so related that the movement is dead-beat. Professor Hermann, starting from Lippmann's polarisation theory, assumes the simplest conceivable relation between the rate of polarisation and the acting P.D., namely, that they are proportional to one another. Putting i = the intensity of the current, and p = the amount of polarisation at the time t , he gets

$$dp/dt = hi,$$

in which h is an instrumental constant.

Writing E for an electromotive force, which may be constant or variable, and w for the resistance of the circuit, he arrives at the differential equation

$$\frac{dp}{dt} = \frac{h}{w} (E - p).$$

* 'Journal of Physiology,' vol. 18, p. 117.

† "Time-Relations of the Capillary Electrometer," 'Phil. Trans.,' A, vol. 183, p. 81, 1892.

My position in relation to the problem was very different. I wanted to make a capillary electrometer from the description given in Lippmann's Theses. In order to get better results, I determined by actual experiment what were the conditions of sensitiveness and rapidity, and in doing this found out so much about the instrument that the "einfachste denkbare Annahme," referred to by Hermann, would not have commended itself to me.

My paper on the "Time-Relations of the Capillary Electrometer" was a condensed account of a small portion of the work done by me. For various reasons I did not then enter into my views as to the theory of the instrument, and will confine myself here to a statement of them, which must be regarded as preliminary.

Professor Hermann speaks of my theory having been empirically obtained. I demur to that expression as open to misconstruction. My working formula may rightly be called empirical, since it neglects certain terms of the complete expression, which I have found to neutralise each other in a suitably selected instrument, but my theory of the time-relations of the capillary electrometer was founded upon first principles and verified by experiments.

My starting point was the fundamental fact that in the capillary electrometer a mechanical effect is produced by an electrical cause. But there are several links between the cause and the effect, and a strong probability that each of them involves a time-function.

They are shown in the following scheme:—

I.	II.	III.	IV.
A difference of potential (the establishment of which is delayed by the (varying) internal ohmic resistance of the electrometer) produces	A change in the constant of capillarity at two interfaces between mercury and an electrolyte.	Presumably giving rise to polarisation at the aforesaid interfaces.	And does work in moving a column of mercury against the force of gravity (with more or less rapidity according to the (varying) amount of fluid friction in the tube).

Poiseuille showed in 1846 that the flow of a liquid through a capillary tube varies directly as the pressure. Of this I was not aware till later, but it leads to precisely the same differential equation as that adopted by Hermann.

Writing Q for the quantity of electricity, C for the constant of capillarity, P for polarisation, and W for the work done, the symbolical expression of the problem is—

$$f(Q_t, C_t, P_t) = \phi(W_t).$$

Hermann has passed over C, and omitted to take W into account, confining himself to the theoretical relation between Q_i and P_i .

But we know very little about polarisation, save in the case of solid electrodes. The term polarisation, as frequently used, includes two phenomena, which ought to be kept distinct, viz. :—

(a) That condition of the interface between two conductors, of which one at least is an electrolyte, in which the molecules are under a stress not greater than they are capable of supporting without chemical change. .

(b) A deposit upon the surface of a solid, or in the contiguous liquid, of the products of actual electrolysis.

If one of the conductors is a solid, the inevitable local differences of condition or of composition enable actual electrolysis to take place, even with a P.D. smaller than that proper to the chemical change implied.

But if both conductors are liquid and perfectly pure, the stress is so far equalised that no electrolysis is possible until the E.M.F. reaches a certain value, more sharply defined in proportion as the materials are pure.

I hold that with differences of potential which do not reach this limit, the electromotive force is transmuted without electrolysis into mechanical force, and manifests itself as kinetic energy, until by the motion of the meniscus it becomes transformed into potential energy.

The locus of transformation from electrical to mechanical force must clearly be the two interfaces mercury-acid and acid-mercury, and it is upon these that the stress acts. The resistance is distributed along the tube, and is partly electrical, but to a far larger extent mechanical.

Is it reasonable, therefore, to assume that the sole cause of delay is the "Polarisations-geschwindigkeit" of the meniscus?

I believe that in the case of an interface between two liquids, the rate of polarisation is to be measured in terms of the vibration-period of a molecule, rather than in decimals of a second.

Actual electrolysis is another matter, and I hold that it does not take place in a properly working electrometer. We do not assume electrolysis when two pith balls repel each other after receiving a charge, nor when a closed coil is slipped over a magnet. But the coil cannot be got off again, nor can the balls fall together without the generation somewhere of a current. I cannot see why we should assume electrolysis in the case of the capillary electrometer. The marvellous rapidity of the action to which I have not yet found a limit, is against it, as is also the fact that the substitution for the acid, or the addition to it, of any substance which can be electrolysed by a smaller electromotive force, reduces the range of potential difference for which it can be used.

The presence of even a trace of impurity is soon manifested by the blocking of the capillary, and if this block is removed by electrolysis, the instrument behaves for some time in an abnormal way. It shows signs of a residual charge, like that of a Leyden jar, the mercury rising again after the short-circuiting key is opened, instead of simply ceasing to fall.

This I ascribe to polarisation of the kind met with between solids and electrolytes, and to this the term "Polarisations-geschwindigkeit" would be applicable. But no good electrometer will show it, except with electromotive forces greater than ought to be employed. I have held from the first that the capillary electrometer acts by transforming electrical into mechanical energy without any chemical interchange, and that this is possible because at the interface between two liquids which do not diffuse into each other the stress is so evenly distributed that no one molecule can be strained to a degree sufficient to detach any part of it until the stress is intense enough to break down all similar molecules simultaneously.

But if by polarisation is meant this condition of the interface, then I maintain that it must precede the movement, and must be developed with almost inconceivable rapidity.

In order to investigate the form of curve produced by recording the motion of the meniscus when the electrometer is acted upon by an electromotive force varying with the time according to some known law, *e.g.*, the pulsating or alternating current of a dynamo, Professor Hermann puts his equation into a somewhat different form, namely :

$$dp/dt + rp - r\epsilon f(t) = 0,$$

where r and ϵ are constants, and $\epsilon f(t) = E$ is the electromotive force represented as a function of the time.

But this is simply my own formula for the estimation of the E.M.F. expressed as a differential equation.

For dp/dt is, in the polar curves taken with my machine, merely the subnormal N , and rp is identical with $k\Delta r$, whence

$$dp/dt + rp = r\epsilon f(t)$$

is identical with

$$N + k\Delta r = \frac{1}{0.0133} f(t) \text{ volt,}$$

which being interpreted signifies

$$\left\{ \begin{array}{l} \text{The sub-} \\ \text{normal} \\ \text{to the} \\ \text{curve.} \end{array} \right\} + \left\{ \begin{array}{l} \text{A constant mul-} \\ \text{tiple of the dis-} \\ \text{tance from the} \\ \text{zero-line.} \end{array} \right\} = \left\{ \begin{array}{l} \text{A constant} \\ \text{multiple of} \end{array} \right\} \left\{ \begin{array}{l} \text{The} \\ \text{E.M.F. at} \\ \text{time } t \text{ (in} \\ \text{volts).} \end{array} \right\}$$

Professor Hermann finds the complete primitive of this differential equation, and then, introducing various values of r and the function $E = \epsilon f(t)$, draws, by a process which is indeed laborious, the curves of the corresponding excursions. My own method gives a good deal of the information so obtained in a much simpler manner.

Adopting the letters used by him, when f vanishes we have

$$dp/dt + rp = 0,$$

that is to say, whenever the E.M.F. falls to zero the reduced values of the subnormal and the radius vector are equal, but of opposite sign, and the curve, therefore, can never come back to the zero line under the action of a current which pulsates but does not alternate (see figs. 2 and 4 in Hermann's paper). When the meniscus crosses the zero line, $rp = 0$, and $dp/dt = r\epsilon f(t)$, i.e., the impressed E.M.F. is then directly proportional to the subnormal. This involves the further fact that the crossing of the zero line by the meniscus must always lag behind the change of sign of the E.M.F.

If dp/dt vanishes, as it does at the apex of a spike or the bottom of a notch, the instantaneous value of the impressed E.M.F. is directly proportional to the distance of the meniscus from zero.

The curves drawn by Professor Hermann are for the most part, so far as the eye can judge, similar to those obtainable under like conditions with the capillary electrometer. I have photographed and analysed many such, using rheotomes and dynamos of various kinds, both alternating and direct current, as sources of E.M.F. I have proposed, in a paper which has been in the publisher's hands since last November, that this method should be used to determine the characteristic current curves of dynamos.*

All the confusing influence of the lag vanishes when such curves are analysed—there is no need to trouble about the equation to the curve, since each several term of its differential equation at any given point is found at once by my mode of analysis. But I must point out that an error has crept into Professor Hermann's rendering of the curve given in fig. 6—or, rather, as it only pretends to be an approximation, that it is not equally accurate throughout. The portion $c'd'$, which corresponds to a diminishing negative (below zero) potential is represented as rising with increasing velocity instead of falling more slowly, as it should do. Yet, when this negative potential ceases, the curve commences to fall from d' to e' along the logarithmic curve of discharge. This is impossible. When $\epsilon f(t)$ is negative, the algebraic sum of dp/dt and rp must be negative also if the fundamental equation holds good. Probably the straight line cd has been placed too far to the right.

* 'The Electrician,' July 17, 1896, *et seq.*

Professor Hermann questions the accuracy of my method of analysis when applied to steep curves.

My answer is that I do not employ it in such cases, preferring to take photographs of sudden changes upon plates moving with sufficient rapidity to suitably develop the curves.

Thus in Professor Burdon Sanderson's paper,* figs. 1, 2, 3, and 4, on Plate 1, and figs. 3, 4, and 5, on Plate 3, were intended to show within the limits of a page the entire course of certain phenomena. I did not analyse them, but simply measured the times of the maxima and minima. The remaining curves, viz., figs. 5 and 6, Plate 1, figs. 1—7 on Plate 2, and figs. 1 and 2 on Plate 3, are all suitable for analysis, with the exception of the first phase of fig. 1, which is almost too steep. I have done some thirty or forty of this kind.

As regards the further criticisms, so far as the physical interpretation of the curves is concerned, I can only say that cases did occur in which the maximum E.M.F. of the second (positive) phase exceeded the maximum E.M.F. of the first (negative) phase of the same response. With respect to curves, like those in figs. 3 and 4, Plate II, the part referred to by Professor Burdon Sanderson as the "hump," is not merely the curve of discharge. The actual negatives which I measured show a *rise* of the meniscus after its rapid downward movement has ceased, and *while it is still above the zero line*, and a similar rise is plainly visible to the eye after every one of the "spikes" in figs. 1, 2, and 4, Plate 1, which were photographed with the machine moving more slowly.

It is impossible for the mercury, under these conditions, after approaching the zero line, to recede from, without crossing it, except under the influence of a *negative Acting P.D.* That is to say, the Impressed E.M.F. must be of the same sign as the charge already in the instrument, but must be of higher potential difference. In some negatives this second rise is followed by a descent more rapid than that of the curve of discharge, and therefore indicating a small positive Acting P.D. I first noticed and called attention to it in connection with the curves illustrating my paper† on the "Time Relations of the Capillary Electrometer," but refrained from discussing its physiological significance.

* 'Journal of Physiology,' vol. 18, p. 117.

† 'Phil. Trans.,' A, vol. 183, p. 104.

“An Attempt to determine the Adiabatic Relations of Ethyl Oxide.” By E. P. PERMAN, D.Sc., W. RAMSAY, Ph.D. F.R.S., and J. ROSE-INNES, M.A., B.Sc. Received November 6,—Read December 10, 1896.

(Abstract.)

The wave-length of sound in gaseous and in liquid ethyl oxide (sulphuric ether) has been determined by the two first-mentioned of the authors, by means of Kundt's method, between limits of temperature ranging from 100° C. to 200° C., and of pressure ranging from 4000 mm. to 31,000 mm. of mercury, and of volume ranging from 2.6 c.c. per gram to 71 c.c. per gram. Making use of the same apparatus throughout, the results obtained are to be regarded as comparative, and, by careful determination of the pitch of the tone transmitted through the gas, it is probable they are approximately absolute.

The sections of the complete memoir deal with (I) a description of the apparatus employed, (II) the method of ascertaining the weights of ether used in each series of experiments, (III) determinations of the frequency of the vibrating rod, (IV) the calculations of the adiabatic elasticity and tables of the experimental results, and (V) a mathematical discussion of the results. The last section is due to Mr. Rose-Innes.

As the theoretical results are of interest, a brief outline of them may be given here.

It will be remembered that one of the authors, in conjunction with Dr. Sydney Young, showed that for ether, and for some other liquids, a linear relation subsists between pressure and temperature, volume being kept constant, so that

$$p = bT - a.$$

It has been found that a similar relation obtains between adiabatic elasticity and temperature, volume, as before, being kept constant; so that, within limits of experimental error, where E stands for adiabatic elasticity,

$$E = gT - h,$$

g and h being functions of the volume only. Between these two equations, we may eliminate T , and so express E as a linear function of p , volume being kept constant. The coefficient of p in such an equation would be g/b , and this fraction, on being calculated from the data available, proves to be nearly constant. For working purposes it is assumed that g/b may be treated as strictly constant, and

it is shown that this assumption does not introduce any serious error within the limits of volume considered. We then find it possible to integrate the resulting differential equation, and the complete primitive enables us to draw a set of adiabatic curves. We believe that this is the first time adiabatic curves have been obtained for any substance except perfect gases.

A mathematical discussion is added as to what extent the equations

$$E = gT - h$$

and

$$g/b = \text{constant},$$

can be considered as strictly true, and not merely approximate.

The experimental results for liquid ether form an appendix to the paper.

“The Chemical and Physiological Reactions of certain Synthesised Proteid-like Substances. Preliminary Communication.” By JOHN W. PICKERING, D.Sc. (Lond.). Communicated by Professor HALLIBURTON, F.R.S. Received November 10,—Read December 10, 1896.

The experiments of Professor Grimaux,* made more than ten years ago, have until recently attracted but little attention amongst English physiologists, although that investigator has synthesised a series of colloidal substances which, in their chemical characteristics, show striking similarities to proteids.

Working alone, and in collaboration with Professor Halliburton, I have shown that three of the substances synthesised, viz., the “Colloids amidobenzoic A and B,” formed by the interaction of phosphorus pentachloride and meta-amido-benzoic acid at 125° C., according to the details described in Grimaux’s papers, and the “colloïde aspartique” formed by the passage of a current of dry gaseous ammonia over solid aspartic anhydride heated to 125° C., not only give the leading chemical reactions of proteids, but when intravenously injected into dogs, cats, or pigmented rabbits, cause extensive intravascular coagulation of the blood, in a manner indistinguishable from the physiological action of nucleo-proteids. When injected into the veins of albino rabbits or into the vascular system

* Grimaux, ‘Comptes Rendus,’ vol. 93, p. 771, 1881; *ibid.*, vol. 98, p. 105, 1884; *ibid.*, vol. 98, p. 1434 and p. 1578.

† Pickering, ‘Journ. Physiol.’ vol. 14, p. 341, 1893; ‘Comptes Rendus,’ vol. 120, p. 1348, 1895; ‘Physiol. Soc. Proc.’ Feb. 16, 1895 (‘Journ. Physiol.’ vol. 17); ‘Journ. Physiol.’ vol. 18, p. 54, 1895; *ibid.*, vol. 20, p. 171, 1896; *ibid.*, vol. 20, p. 310; Halliburton and Pickering, ‘Journ. Physiol.’ vol. 18, p. 285, 1895.

of the Norway hare (*Lepus variabilis*), during its albino condition, these substances fail to induce intravascular coagulation of the blood, although they hasten the coagulation of the blood when drawn from the carotids, in a precisely similar manner to nucleo-proteids.

Taking these facts as the basis of my investigations, I have endeavoured to synthesise substances which will approach more nearly in their chemical and physiological reactions to proteids than those briefly described above; and to further investigate the properties of Grimaux's colloids.

I. General Description of Experiments.

I have up to the present synthesised seven different colloidal substances, by the interaction of either phosphorus pentachloride or pentoxide on certain well-known derivatives of proteids, and the details of their preparation, physical properties, chemical and physiological reactions are described below.

Colloid α.—Prepared by the interaction of equal parts of meta-amido-benzoic acid, biuret, and three times its weight of phosphorus pentoxide at 125° C. in a sealed tube. The best results are obtained by continuing the heating for about six hours, although a similar substance is obtained by heating for half an hour at 130° C. The product of the reaction is a pinkish-grey friable powder, which is insoluble in cold water, and almost insoluble in boiling water. This substance should be repeatedly washed until all traces of phosphoric acid are removed. When heated with Millon's reagent it fails to give the reaction characteristic of tyrosine and proteids; it also does not give the well-known colour reactions with the salts of copper, nickel, cobalt, and caustic potash. It gives the typical blue reaction associated with the name of Fröhde* when heated with sulphuric and molybdic acids, as well as the xantho-proteic reaction.

If the amount of biuret exceeds the amount of meta-amido-benzoic acid, then the excess of biuret left over gives its typical colour reaction with copper sulphate and potash.

The pinkish-grey powder, obtained by the reaction described above, should be dissolved in ammonium hydrate, and the resulting solution evaporated down at the temperature of the atmosphere *in vacuo*, when the resulting product appears as a number of translucent yellowish plates, which are tasteless and inodorous, and closely resemble in appearance both Grimaux's "colloïdes amido-benzoïque and aspartique" and dried serum-albumen. These plates are with difficulty soluble in cold water, but readily pass into solution on warming. The solution obtained does not coagulate on heating, but

* Fröhde, 'Annalen der Chemie,' vol. 145, p. 376.

if a trace of a soluble salt of either barium, strontium, calcium, magnesium, or sodium be added, a pronounced coagulum is obtained on heating. This point will be returned to you in a subsequent section, but the similarity to dialysed serum-albumen may be pointed out, as that substance is stated not to coagulate when heated.*

The solution does not coagulate spontaneously on standing, neither will the addition of "fibrin ferment (*i.e.*, a nucleoproteid†) induce coagulation. It gives a typical xanthoproteic reaction, a violet with copper sulphate and potash, a dark heliotrope-purple with cobalt sulphate and potash, and a faint yellow with nickel sulphate and potash. It also gives Fröhde's sulpho-molybdic reaction; I may, however, remark that I found that several substances chemically allied to proteids yield this reaction, which is therefore not diagnostic of proteids alone. An alcoholic solution of alloxan gives with the solid plates a brilliant red coloration (Krasser's‡ reaction) similar to that produced with plates of serum-albumen. Negative results were obtained with the reactions associated with the names of Liebermann,§ Adamkiewicz,|| and Millon.¶

The solution is neutral and lævorotatory ($\alpha_D = -52$), and if treated with pepsin and a 0.2 per cent. hydrochloric acid, or by an alkaline solution of trypsin, for several days at 38° C. it does not peptonise.

Qualitative analysis shows that this substance does not contain phosphorus in its molecule.

It is precipitated from solution by mercuric chloride, silver nitrate, and lead acetate. These precipitates yield the same colour reactions as the original substance.

The precipitate formed by the addition of lead acetate, like that obtained by the addition of this substance to a proteid solution, redissolves on the passage of a current of sulphuretted hydrogen through the solution in which it is suspended, and judging by chemical tests alone, the nature of the substance is unchanged by the processes of precipitation and redissolving. Its physiological action is, however, markedly changed, as will be shown later on.

The original solution is readily precipitated by trichloroacetic, phosphotungstic, phosphomolybdic acids, and by acetic acid and potassium ferrocyanide, as well as by salicylsulphonic acid; the precipitate formed by this last substance is coagulated by heating in a manner similar to the coagulation produced by heating the precipitate resulting from the addition of this substance to a proteid

* Schmidt and Aronstein, 'Pflüger's Archiv,' vol. 8, p. 75, 1874.

† *Vide* Halliburton, 'Journ. Physiol.,' vol. 18, p. 306, 1895.

‡ Krasser, 'Monat. für Chem.,' vol. 7, p. 673; 'Maly's Jahresb.,' vol. 16, p. 1.

§ Liebermann, 'Maly's Jahres.,' vol. 18, p. 8.

|| Adamkiewicz, 'Ber. d. deut. Chem. Gesell.,' vol. 8, p. 761.

¶ Millon, 'Comptes Rendus,' vol. 28, p. 40.

solution. I may here mention that salicylsulphonic acid does not precipitate disintegration products of proteids like leucine, tyrosine, xanthine, or hypoxanthine.

All the precipitates cited above give the colour reactions characteristic of the original substance.

If the original solution is saturated with either magnesium sulphate, ammonium sulphate, or sodium chloride, the whole of the colloid rises to the surface of the liquid, and may be skimmed off. On placing this scum in an excess of distilled water, it rapidly redissolves, forming a pale yellow opalescent solution, which gives all the chemical reactions characteristic of the original substance. If the amount of neutral salt be insufficient to produce precipitation, the passage through the liquid of a current of carbon dioxide or of sulphur dioxide will effect the same result. Neither of these gases will, however, cause precipitation in the entire absence of salts.

The following experiments illustrate the results produced by the intravenous injection of this substance into dogs, rabbits, and cats. The procedure adopted was identical with that described in the previous papers published by Professor Halliburton and myself,* on the intravascular injection of Grimaux's colloids. In all cases the animal was anæsthetised by a mixture of chloroform and ether, an excess of the latter substance being used when the subjects were dogs.

Experiment 1.—Fox terrier (weight 27 lbs. 10 oz.); 25 c.c. of a 0.75 per cent. solution of the colloid α was injected, and proved fatal. Pronounced exophthalmos and dilatation of the pupils, and typical stretching movements were observed.

Post-mortem examination made immediately after death revealed pronounced clots in the jugular vein, inferior vena cava, and portal vein, and a slight clot in the left ventricle and in the pulmonary artery.

Experiment 2.—Large black cat (weight 9 lbs. 6 oz.); 40 c.c. of the colloid proved fatal, with similar symptoms as above. Immediate *post-mortem* examination showed pronounced clots in the left ventricle, right auricle, inferior vena cava, portal, and jugular veins. The remainder of the blood was fluid, but coagulated very rapidly after withdrawal.

Experiment 3.—Black rabbit; 38 c.c. of the same substance produced a similar result.

Experiment 4.—Albino rabbit; 42 c.c. proved fatal. Death was accompanied by pronounced exophthalmos and dilatation of the pupils and stretching movements of the limbs. *Post-mortem* examination showed the blood throughout the vessels to be fluid. It, however, rapidly coagulated after withdrawal from the vessels, and the coagulability of samples of the blood taken from the carotids during

the injection of the colloid was also hastened; thus after 20 c.c. of the colloid had been injected, the time of complete coagulation of blood withdrawn from the carotids was hastened by 2 minutes, after 30 c.c. by $3\frac{1}{2}$ minutes, and after 35 c.c. by 4 minutes.

It will be evident that the results recorded above are similar to, if not indistinguishable from, those produced by the intravenous injection of a nucleoproteid.

When slowly introduced into the circulation of dogs, and to a much lesser degree of rabbits, in minute quantities, the effect produced on the coagulability of the blood is the converse of that resulting from the introduction of larger quantities. This effect is more pronounced than that obtained by the intravenous injection of Grimaux's colloids, and more resembles Wooldridge's* "negative phase," which is characteristic of a nucleoproteid, but is not so pronounced as the result obtained with that substance.

This result is illustrated by the following experiment:—

Experiment 5.—Large black mongrel. Anæsthetic, ether and morphia (weight, 60 lbs.); 1 c.c. of a 0.025 per cent. solution colloid α was injected very slowly, the injection being distributed over half an hour, at the end of which time the retardation of the time of coagulation of blood withdrawn from the animal's carotid was found to be 8 minutes 30 seconds. A second dose of 1 c.c. of the same solution injected and distributed over 20 minutes caused a further retardation in the time of coagulation of the carotid blood of 2 minutes; but a third injection distributed over a similar period of time hastened the coagulability of the blood that had been previously retarded, so that the retardation, as compared with the time of coagulation before the injection of the colloid, was only 1 minute 30 seconds. After a still further injection of the colloid, the blood coagulated more rapidly than in the normal condition, and finally, when the dose was pushed, intravascular coagulation of the animal's blood occurred, and death resulted.

If the colloid is separated from the solution by saturation with magnesium sulphate, sodium chloride, or ammonium sulphate, as before described, and the scum redissolved in distilled water, the opalescent solution obtained will, when intravenously injected into pigmented rabbits, produce typical intravascular coagulation. Repetition of the process of precipitation and redissolving however, destroys the physiological activity in a manner similar to the result produced with both nucleo-proteids and Grimaux's synthesised colloids.

If the solution formed by the passage of a stream of sulphuretted hydrogen over the precipitate formed by the addition of lead acetate to the colloid is injected intravenously into pigmented rabbits or

* Wooldridge, 'Du Bois-Reymond's Archiv,' 1886, p. 397; 'Proc. Roy. Soc.,' vol. 40, p. 134, 1886.

dogs, it is found not to induce intravascular coagulation, although its chemical and physical characteristics are apparently unchanged. This result shows that the chemical reactions used for "testing" proteids are not sufficiently delicate to indicate the chemical changes which are demonstrable by physiological methods. The following experiment illustrates this result:—

Experiment 6.—Black rabbit (weight 7 lbs. 9 ozs.); anæsthetic, chloroform and ether; 120 c.c. of redissolved solution injected produced dyspnœa, exophthalmos, dilatation of pupils. A further injection of 10 c.c. of this substance was immediately fatal. *Post-mortem* examination failed to reveal any clots in the animal's vessels. Blood withdrawn from the carotids during the injection showed only one minute's decrease in the time taken to complete coagulation.

Experiment 7.—In another experiment, where minute quantities of this substance were very slowly injected, there was no retardation of the time of coagulation, like that produced by the original substance or by a nucleo-proteid.

Colloid β .—This substance is formed by heating together tyrosine, biuret, and phosphorus pentachloride in the ratio of equal weights of the two former substances, with twice the weight of the latter, for six hours at 125° to 130° C. in sealed tubes. The product of this reaction is a grey powder insoluble in cold water, and very sparingly soluble on heating. This substance gives the xantho-proteic and Fröhde's reaction, but fails to give typical colour reactions with the other reagents commonly used in testing proteids. It should be repeatedly washed until all traces of the contaminating phosphoric acid are removed, and then dried *in vacuo* at about 30° C. It readily dissolves in concentrated ammonium hydrate, and the solution is opalescent and lævorotatory ($\alpha_D = -48$), and in appearance indistinguishable from that of the other colloids produced. It gives the following distinctive reactions as classified in the annexed table, but does not digest when subjected to the action of either pepsin and 0.2 per cent. hydrochloric acid for three days at 38° C., or of an alkaline solution of trypsin, kept at the same temperature for a similar time. It yields the following distinctive reactions:—

Colloid β .

CuSO ₄ KHO.	CoSO ₄ KHO.	NiSO ₄ KHO.	H ₂ SO ₄ and molybdic acid.	Millon's reagent.	HNO ₃ and NH ₄ OH (heating).	Salicyl sulphonic acid.
Violet- coloured solution.	Heliotrope purple- coloured solution.	Faint yellow- coloured solution.	Dark blue precipitate.	Dark red precipitate.	Orange precipitate.	Precipitate which coagulates on heating.

It gives negative results with the reactions of Liebermann and Adamkiewicz, but gives the typical red coloration when the solid plates are heated with an alcoholic solution of alloxan (Krasser's reaction). It is separated from solution by neutral salts in a manner similar to the colloid α and Grimaux's colloids. The scum also redissolves in distilled water giving an opalescent straw-coloured solution. It is precipitated by silver nitrate, lead acetate, and mercuric chloride, as well as by phosphotungstic, phosphomolybdic, and trichloroacetic acids, and by acetic acid and potassium ferrocyanide.

In the entire absence of salts it is not coagulated on boiling, but, on the addition of a trace of a soluble salt of either sodium, magnesium, barium, strontium, or calcium, a coagulum is obtained on heating to 74° C.

The fractional heat coagulation of this substance will be dealt with in a subsequent section.

The effect produced by the intravascular injection of various quantities of this body is illustrated by the following experiment:—

Experiment 8.—Brown mongrel (weight 27 lbs. 7 oz.); anæsthetised with ether and morphia. The jugular vein on the one side, and the carotid artery on the other, were exposed, and cannulae inserted into them. The colloid β was injected into the jugular vein, and samples of blood withdrawn from the artery. The following table shows the rate of clotting of the various samples:—

- (1) Before injection of the colloid, the blood clotted in 10 minutes 30 seconds.
- (2) 5 c.c. of 0.75 per cent. solution of colloid dissolved in 0.75 per cent. saline injected. A firm clot formed in 17 minutes 8 seconds.
- (3) 10 c.c. more injected. Loose clot in 22 minutes.
- (4) 10 c.c. more injected. Firm clot in 31 minutes.
- (5) 10 c.c. more injected. Firm clot in 13 minutes.
- (6) After interval of 5 minutes a second sample of carotid blood formed a firm clot in 7 minutes 30 seconds.
- (7) 7 c.c. more injected. Firm clot in 7 minutes 30 seconds.
- (8) 10 c.c. more injected. Firm clot in 6 minutes.
- (9) 15 c.c. more injected. Firm clot in 3 minutes.
- (10) 10 c.c. more injected and proved fatal.

Immediate *post-mortem* examination revealed loose clots in vena cava inferior, and jugular vein, and pronounced clots in portal vein, and right ventricle.

This experiment shows the "negative phase" after injection of small quantities of the colloid β , and the typical hastening of the coagulability of the blood withdrawn from the carotid after the intravenous injection of

a larger dose, and finally the coagulation of the intravascular blood when the dose is again increased.

Colloid γ .—The colloid γ is formed by heating together at 130°C . in sealed tubes, for three hours equal weights of alloxan and metamido-benzoic acid, with twice their weight of phosphorus pentoxide. The product of the reaction is a white powder, very slightly soluble in cold water, and sparingly soluble in warm water. It should be washed in ice-cold water till the excess of phosphoric acid is removed, and the remaining substance dissolved in concentrated ammonia. The resulting solution is opalescent and straw-coloured, and should be evaporated down at the temperature of the laboratory *in vacuo*, when a number of translucent, yellowish plates, closely resembling the previously described colloids are formed. These plates are soluble in warm water, and the solution is pale straw-coloured, opalescent, and lævorotatory ($\alpha_D = -41$) and shows the following reactions:—

Colloid γ .

HNO_3 NH_4OH . (heating).	Millon's reagent.	Fröhde's reaction.	CuSO_4 and KHO .	NiSO_4 and KHO .	CoSO_4 and KHO .	Salicyl- sulphonic acid.
Yellow solution.	Dirty brown ppt.	Blue pre- cipitate.	Violet solution.	Very faint yellow solution.	Dark brown solution.	No pre- cipitate.

It is separated from solution by saturation with either magnesium sulphate, sodium sulphate, sodium chloride, or ammonium sulphate, the colloid rising to the surface of the liquid as a white scum, which redissolves, forming an opalescent solution when thrown into distilled water. It is precipitated by silver nitrate, lead acetate, and mercuric chloride. If the precipitate formed by the addition of lead acetate is suspended in distilled water, and a current of sulphuretted hydrogen is passed through the liquid, the precipitated colloid again passes into solution.

When heated in the presence of a trace of a neutral salt, fractional heat-coagulation is obtained, which will be detailed in a subsequent section.

If the colloid γ is injected into the circulation of dogs or pigmented rabbits, even in large quantities, it does not produce intravascular coagulation, although it somewhat hastens the coagulability of blood withdrawn from the carotid.

The colloid γ , although yielding many of the chemical reactions that have been used as distinctive tests for proteids, and also behaving in a very similar manner to the previously described proteid-like colloids, does

not, like them, produce intravascular coagulation when intravenously injected into pigmented rabbits. Neither will the colloid γ when introduced into the circulation of dogs, very slowly and in minute quantities, produce a retardation of the coagulation of blood withdrawn from the carotids.

Colloid δ .—The colloid δ is formed by heating at 125° C. in sealed tubes for three hours, equal weights of para-amidobenzoic acid and phosphorus pentachloride. The resulting product, a grey friable powder, insoluble in cold water, was, after washing to remove the contaminating phosphoric acid, dissolved in concentrated ammonia, and evaporated down at a low temperature *in vacuo*. The resulting substance appears as a number of translucent yellowish plates, apparently similar to those previously described. They are soluble in warm water, forming an opalescent straw-coloured solution, which is lævorotatory ($\alpha_D = -42$). This solution gives the xantho-proteid and Fröhde's reaction, but fails to give the typical colour reactions of proteid-like substances with salts of copper, cobalt, or nickel and caustic potash; neither does it give the reactions of Millon, Liebermann, or Adamkiewicz. It is not precipitated by salicylsulphonic acid, but it is precipitated by salts of the heavy metals. Neutral salts separate it from solution like the preceding substances. When freed from salts, it does not coagulate on heating, but if a trace of sodium chloride or of another neutral salt be present, it coagulates on heating to 75° C. When intravenously injected into pigmented rabbits, it fails to produce intravascular coagulation, neither does it hasten the coagulability of blood withdrawn from the carotids. It fails to induce a "negative phase" in the coagulation of dogs' blood. This series of results lends additional support to the view that the coagulation of the blood resulting from intravenous injection of the colloid, is due to the interaction of the colloid with the constituents of the plasma, and not to the heavy nature of colloid molecule.

Colloid ϵ .—The colloid ϵ is prepared by heating together equal weights of tyrosine and xanthine with twice their weight of phosphorus pentachloride at 125° C. for three hours. The product of the reaction is a yellowish powder slightly soluble in warm water. After repeated washing in cold water, it is dissolved in concentrated ammonia, and the resulting solution evaporated down *in vacuo* at a low temperature. The resulting substance consists of a number of translucent yellowish plates like those previously described. It is readily soluble in warm water, forming a yellowish opalescent solution, which is lævorotatory ($\alpha_D = -38$).

This solution gives a typical red when heated with Millon's reagent, which is not due to an excess of tyrosine, since the intermediate product in the preparation of the substance fails to give this reaction. It does not give any other of the distinctive proteid colour reactions,

but is precipitated by salicylsulphonic acid, and the precipitate coagulates on heating. It behaves with neutral salts and salts of the heavy metals similarly to the previously described substances. It does not cause intravascular coagulation of the blood when intravenously injected into dogs or pigmented rabbits, neither will the very slow injection of minute quantities into the circulation of dogs induce a "negative phase." It does not induce coagulation when added to 1 per cent. sodium carbonate plasma.

Colloid ζ is prepared in a similar manner to the colloid ϵ , hypoxanthine being substituted for xanthine. It has a similar appearance to the colloid ϵ , is lævorotatory ($\alpha_D = -40$), gives Millon's reaction, and negative results with the other tests characteristic of proteids.

It also behaves with neutral salts and salts of the heavy metals in a similar manner to the previously described substances. When intravenously injected into the circulation of dogs or pigmented rabbits, it fails to induce intravascular coagulation, neither will it cause coagulation when added to extravascular 1 per cent. sodium carbonate plasma.

Colloid η .—The colloid η is prepared by the interaction of tyrosine and phosphorus pentoxide for three hours at 130° C. in sealed tubes. The product of this reaction is a pinkish friable powder, sparingly soluble in cold water and soluble on boiling. This substance does not yield Millon's reaction. After washing in cold water to remove the contaminating phosphoric acid, the powder is dissolved in concentrated ammonia, and a straw-coloured opalescent solution is obtained. This is evaporated down *in vacuo*, and the resulting substance appears as a number of plates, similar in appearance to those of the previously described colloids, and which are soluble in warm water, giving an opalescent solution. This solution is precipitated by salicylsulphonic acid and the precipitate coagulates on heating. It is also precipitated by salts of the heavy metals, and separated from solution by neutral salts. It does not yield any of the distinctive colour reactions of proteids, and fails to produce intravascular coagulation when intravenously injected into rabbits.

II. *The Fractional Heat Coagulation of Synthesised Colloids.*

The method of differentiating the members of a mixture of proteids by fractional heat coagulation was introduced by Halliburton,* and employed by him more especially in the examination of the proteids of serum. This method was subsequently used by Corin and Bérard† in separating the albumins of the white of egg, and by Chittenden

* Halliburton, 'Journ. Physiol.,' vol. 5, p. 159.

† Corin and Bérard, 'Bul. de l'Acad. Roy. de Belgique,' vol. 15, 4, 1888.

and Osborne* in studying the proteids of maize. The method was rendered more accurate by Hewlett,† who substituted a bath of cod-liver oil for the water bath usually employed as the heating medium, and exhaustively dealt with the adverse criticisms made by Haycraft and Duggan.‡

I have applied this method, using an oil bath, in the examination of the proteid like colloids synthesised by Professor Grimaux and myself. As pointed out in a previous section, in the entire absence of salts these substances do not coagulate, even when boiled. For the sake of comparison the following experiments were performed, so as to satisfy the following conditions:—(a) A 2 per cent. solution of the substance under examination was always used. (b) The diluent fluid always consisted of a 0.75 per cent. solution of sodium chloride. (c) In each experiment 10 c.c of the fluid under examination was used, and the test-tubes were of uniform internal diameter. By this means the mass to be heated remained constant. (d) The thermometer was placed in the middle of the test-tube containing the fluid under examination.

The colloid A (*"colloïde amidobenzoïque"* of Grimaux) shows a coagulation temperature of 70° to 71° C.

The colloid B (of Grimaux) which is prepared from the same reagents as the colloid A, but the temperature at which the reaction of synthesis is conducted is allowed to rise to 130° C., shows on heating one faint appearance of flocculi at 56° to 58° C., and a second more pronounced coagulum at 70° to 72° C.

The colloid C (*"colloïde aspartique"* of Grimaux) on fractional heating shows three distinct sets of flocculi, appearing respectively at 58°, 67°, and 73.1° to 76.4° C.

The colloid α , if care has been taken to keep the temperature of preparation constant at 125° C., shows, on heating, only one coagulum at 70.6°; if, however, in the preparation of this colloid the temperature of synthesis is allowed to rise, a second colloid coagulating at 42° C. is often but not always formed.

The colloid β , even when the temperature of the synthesis has been kept constant at 130° C., shows, on heating, three constituents coagulating at 47° C., 56° C., and 74° C.

The colloid γ apparently only has one temperature of heat coagulation, viz., 75° C.

The colloid δ coagulates at 75° C.

The colloid ϵ coagulates only at 47° C.

The colloid ζ coagulates at 48° and 59° C.

* Chittenden and Osborne, 'Amer. Chem. Journ.' vol. 13, 7 and 8; vol. 14, 1.

† Hewlett, 'Journ. Physiol.' vol. 13, p. 493, 1892.

‡ Haycraft and Duggan, 'Brit. Med. Journ.', 1890, vol. 1, p. 167; 'Edin. Roy. Soc. Proc.', vol. 16, p. 361, 1888-9.

The colloid η coagulates only at 52° C.

Adopting the conclusion of Halliburton that the precipitates obtained by the fractional heat coagulation of a proteid substance, correspond with various constituents of that substance, we may possibly conclude that those synthesised colloids which yield fractional heat-coagula are mixtures of different colloidal substances.

Thus the colloid B would consist of two substances which might be designated B_1 and B_2 , and the colloid β of three substances, designated colloids β_1 , β_2 , and β_3 respectively, and the colloid δ of two substances, δ_1 and δ_2 . I have endeavoured to ascertain in the cases of the colloids B_1 and B_2 and of the colloids β_1 , β_2 , and β_3 whether each of these substances will equally induce intravascular coagulation of the blood, when intravenously injected into pigmented rabbits and dogs.

The method of procedure adopted was briefly as follows:—The activity of a solution of the colloid was tested by a control experiment. One of the constituents was removed by fractional heat-coagulation and the effect, if any, produced by the intravascular injection of the remaining colloid in solution was tested.* The following is the record of some of the results obtained:—

Colloid B_2 after a removal of colloid B_1 will, if intravenously injected, induce intravascular coagulation in pigmented rabbits, and if slowly injected in minute doses a “negative phase” in dogs.

Colloids β_2 and β_3 will still, after the removal of colloid β_1 , induce intravascular coagulation in pigmented rabbits, although a much larger dose is required after the removal of β_1 and β_2 than if the mixture of the three substances is injected, if only β_1 is removed the activity of the mixture is not impaired. From this I conclude that β_2 and β_3 are the active constituents of the colloid mixture I have designated as the colloid β . There is apparently no difference in the tendency to induce a “negative phase” in dog’s blood after the removal of β_1 and β_2 from the solution.

III. *Other Properties of the Synthesised Colloids.*

The influence of these substances on red and white blood corpuscles, and on extravascular 1 per cent. sodium carbonate plasma will be described in a subsequent paper.

IV. *Concluding Remarks.*

It is evident from the observations recorded in the preceding pages, that if certain derivatives of proteids, and other substances of

* The solution after removal of one of its constituents by fractional heat-coagulation, was evaporated down *in vacuo* until it had the same specific gravity as the original solution.

allied chemical constitution are heated together in sealed tubes with an excess of either phosphorus pentachloride or pentoxide, a series of colloidal substances are formed which, when freed from the contaminating phosphoric acid, and dissolved in concentrated ammonia, give opalescent solutions that, on evaporating down *in vacuo*, yield substances closely resembling in physical, chemical, and physiological properties certain proteids.

These colloidal substances, although they differ from one another in minor details, are usually distinguished by the following characteristics :—

1. They are soluble in warm water, forming opalescent laevorotatory solutions.

2. The resulting solutions yield the principal colour reactions hitherto deemed diagnostic of proteids.

3. In the absence of salts, solutions of these colloids do not coagulate on heating. In the presence of a trace of a neutral salt they coagulate on heating at temperatures very similar to proteid solutions.

4. Fractional heat-coagulation shows the colloidal solutions are a mixture of different substances.

5. The different constituents of the colloidal solution exhibit different physiological action.

6. In the presence of an excess of neutral salts, or of salts of the heavy metals, the colloidal solutions behave in a manner similar to proteid solutions.

7. When introduced into the circulation of pigmented rabbits, dogs, and cats, certain of these substances (*viz.*, the colloids designated A, B, C, α and β) produce intravascular coagulation of the blood in a manner similar to a nucleo-proteid. They also hasten the coagulability of the blood withdrawn from the carotid, and will, when slowly injected intravenously in minute quantities into dogs, produce a retardation of the coagulability of the intravascular blood, *e.g.*, a "negative phase."

8. Apparently these colloidal substances are, owing to both their physical and chemical properties and their physiological behaviour, the nearest synthesised bodies at present known to proteids.

"An Experimental Examination into the Growth of the Blastoderm of the Chick." By RICHARD ASSHETON, M.A.
Communicated by ADAM SEDGWICK, F.R.S. Received
November 12,—Read December 10, 1896.

In making an experimental study of the growth of the blastoderm of the chick, I had two chief objects in view :

- (1) To test by actual experiment Duval's* theory of the formation of the primitive streak.
- (2) To try and determine experimentally whether the whole or only part of the actual embryo is developed by the activity of the primitive streak. And further, if only a part, to determine its limits.

With regard to the first question it may be remarked that Duval's account is generally accepted, although perhaps greater stress is laid upon it by foreign and American writers than by embryologists in this country.

According to Duval's account, there is in the freshly laid and unincubated egg a groove which separates the blastoderm from the yolk. The groove, he says, is broader and more conspicuous at the posterior margin than at any other point. This he compares to the anus of Rusconi or blastopore of the segmenting frog's egg.

During the first few hours of incubation the edge of the blastoderm is said to advance over the yolk at every point except at this most posterior margin bounding the groove, which he regards as equivalent to the frog's blastopore. At this spot there is no advance. The portions of the edge of the blastoderm adjoining this part swing round to meet each other in the middle line, and eventually fuse and form what Duval calls the "plaque axiale."

This structure is in reality the primitive streak, and, according to Duval, it becomes visible as such during about the tenth to fifteenth hours of incubation by reason of the subsequent hollowing out of the subjacent yolk by the extension backwards of the sub-germinal cavity.

Such a mode of growth would be very extraordinary and interesting if true, and would be very acceptable to those who believe that the growth in length of the vertebrate embryo is caused by a concrescence of two at first separated germinal rims.

Naturally this account of the formation of the primitive streak as given by Duval is frequently quoted by the many adherents to the concrescence theory.

During the last few years experimental methods have been introduced much more freely into investigations of animal development. Foremost amongst the workers upon these lines is Dr. Wilhelm Roux, who experimented by destroying certain cells of the segmenting eggs of frogs, and noting the result after some days of development. He has been followed in similar work by Morgan and Ume Tsuda and others.

The eggs of frogs have been the object of experiment of a different

* "De la Formation du Blastoderme dans l'Œuf d'Oiseau," *Annales des Sciences Naturelles, Zoologie*, vol. 18.

kind, such as that of Professor Oscar Hertwig, who studied the various monstrosities obtained by mechanical compressions, by supermaturation of the ovum, and addition of various salts to water in which the eggs were developing. Similar work has been done upon sea-urchin's eggs by several biologists (Pouchet and Chabry, Herbst, &c.).

There are other most valuable records of the results obtained by separating the several spheres of the early stages of segmentation of eggs of Ctenophores, Echinoderms and Amphioxus by Chun, Driesch, Wilson, and others.

Kastschenko, by injuring portions of the germ ring of Elasmobranch embryos, has produced very valuable evidence in connexion with the concrescence theory, and Morgan has by similar methods examined the development of Teleosteans.

As far as I know, an experimental study of the development of the avian blastoderm has not hitherto been made.

The method adopted, which is very simple, was as follows. The egg was first of all opened at one side, and a bristle inserted into the yolk at some distance away from the blastoderm, to mark its anterior and posterior axis.

The yolk, with its surrounding albumen, was then turned out into a glass vessel having a rather greater capacity than that of an ordinary egg shell.

The yolk was arranged so that the blastoderm floated uppermost, and a wire or celluloid ring was placed over it to prevent the yolk from floating to the surface.

A fine sable hair was then inserted in the blastoderm, and its position measured by a micrometer eye-piece and recorded in tenths of a millimetre. The vessel was filled up with albumen and covered with a glass lid, and placed in the incubator at a temperature of 104° F.

Under these conditions, although development was slower than under normal conditions, many embryos reached, after about forty-eight hours, an age equivalent to a normal thirty to thirty-six hours' chick with nine or ten pairs of mesoblastic somites.

To come now to the results of the experiments, it is clear that if Duval's theory is correct a hair inserted in the area opaca at the point a (fig. A (i)) ought to appear, in a specimen in which the primitive streak is formed, somewhere in front of the primitive streak. It, however, does not; it appears in the area opaca behind the primitive streak at a, fig. A (ii).

So again if the primitive streak is formed by the concrescence of the posterior margin, the sables inserted at the posterior edge at XX should either appear in the primitive streak or else prevent its formation.

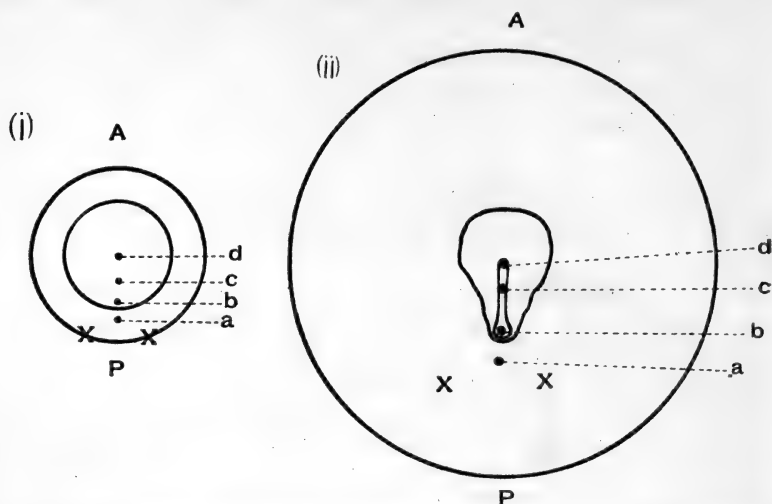


FIG. A.—(i) Diagram of the unincubated Blastoderm of a Bird. (ii) Diagram of the Blastoderm after the complete Formation of the Primitive Streak.

On the contrary, they are found far behind the primitive streak in the area opaca.

These facts seem to show that the primitive streak is not formed from the posterior edge of the blastoderm as Duval maintains.

As a rule, in the unincubated blastoderm the area opaca and area pellucida are very fairly well defined.

If, when this is the case, a sable hair is inserted just within the area pellucida at the point b, or if, when there is no such distinction, the sable is inserted about one quarter the distance from P to A, the sable hair is found, after the development of the primitive streak, piercing the posterior end of the primitive streak—whereas, according to Duval's account, it ought to be somewhere in front of the primitive streak.

If a hair is inserted in the median line rather further towards the centre of the blastoderm, it is found near the middle of the primitive streak, or, if placed about half way between the inner edge of the posterior part of the area opaca and the centre of the blastoderm (as at c), it is found in the anterior part of the primitive streak; and, when the sable is inserted at the centre of the blastoderm, it appears at the front end, or just in front of the primitive streak (fig. A, d).

The foregoing proves, I think, conclusively, that the primitive streak is developed from that portion of the unincubated blastoderm which lies between the centre of the blastoderm and the posterior

margin of the area pellucida. The area opaca takes no part at all in the formation thereof.

I may add that from a careful examination of surface views of living and preserved specimens, and from sections, I find it just as difficult to corroborate Duval's account of the formation of the primitive streak as I do from the experimental study I have just described.

I come now to the second part of the inquiry ; namely, what part of the actual embryo does the primitive streak give rise to ?

A sable hair inserted at the centre of the blastoderm appears at the anterior end of the primitive streak.

If such a specimen is allowed to develop for some hours longer, until the medullary plate and medullary groove are clearly formed, these structures are found to be in front of the sable hair ; that is to say, the sable hair is still at the front end of the primitive streak (fig. B). If a specimen, in which the sable hair has been inserted at the same spot—that is to say, at the centre of the unincubated blastoderm—is left until several pairs of mesoblastic somites have appeared, the hair is found at the level of the most anterior pair of somites (fig. B (iii)).

From these specimens it seems clear that all those parts in front of the first pair of mesoblastic somites (that is to say, the heart, the brain, and medulla oblongata, the olfactory, optic and auditory

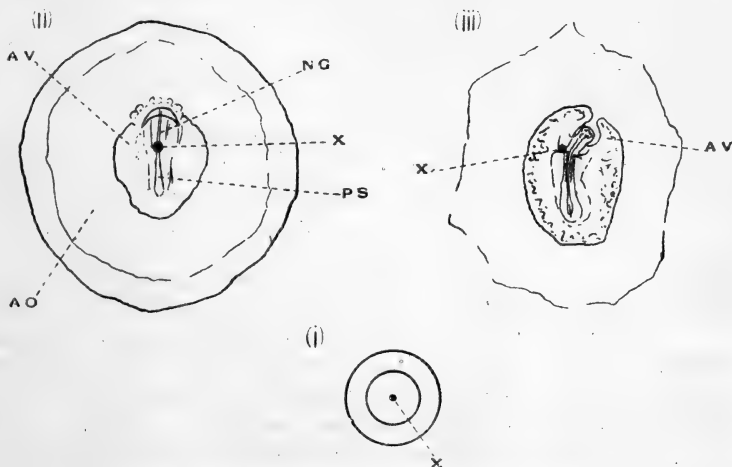


FIG. B.—(i) Diagram of unincubated Blastoderm.
(ii) Blastoderm after 24 hours' incubation.
(iii) Blastoderm after 40 hours' incubation.

AO, area opaca ; AV, area vasculosa ; NG, medullary groove ; PS, primitive streak ; X, point of insertion of sable hair.

organs and fore gut) are developed from that portion of the unincubated blastoderm which lies anterior to the centre of the blastoderm, and that all the rest of the embryo is formed by the activity of the primitive streak area.

I have found it very difficult to determine, exactly, the anterior limits of the embryo in the unincubated blastoderm. This, no doubt, is due to the fact that, for the production of the anterior end of the embryo, very complicated foldings of the blastoderm are called into play, and the insertion of a bristle or the infliction of any injury to the delicate parts of the blastoderm involved in the process, almost entirely prevents anything like a normal course of development.

However, such little success as I have had gives the following results:—A hair inserted at the most anterior border of the area pellucida is found far in front of the primitive streak.

A hair inserted only slightly in front of the centre of the blastoderm appears (in a specimen in which the medullary folds are just becoming visible) in the medullary plate in front of the primitive streak. In older specimens, after the head-fold has been formed, the embryos are extremely abnormal when the sable has been inserted in the region under discussion.

Indeed, very few will develop as far as the formation of the head-fold.

The only facts I can derive from the insertion of sable hairs in this area are:—

- (1) That it interferes very seriously with the course of development.
- (2) That the bristle appears inside the two anterior horns of the area vasculosa.
- (3) That if placed some little way anterior to the centre it is found apparently in front of the embryo, but it interferes so greatly with the head-fold that it is difficult to say whether it has, or has not, perforated the anterior part of the embryo.

I have shown that a hair inserted between the centre of the blastoderm and the hinder margin of the area pellucida is found after about twenty hours of incubation in the primitive streak. When a specimen in which the sable has been similarly placed is allowed to develop until several mesoblastic somites have been formed, it is found to be posterior to the first formed mesoblastic somites.

For instance, in the specimen with me, the blastoderm measured 4.3 mm. in diameter. The sable was inserted 1.3 mm. from the posterior edge of the blastoderm. After forty-one hours of incubation seven pairs of mesoblastic somites had been formed, and the sable hair was a short distance posterior to the 7th pair of somites.

From such specimens as these we are, I think, bound to conclude

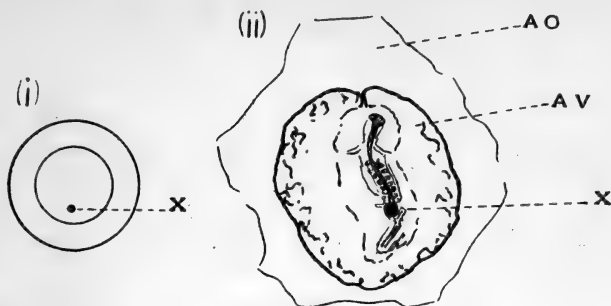


FIG. C.—(i) Diagram of unincubated Blastoderm.

(ii) Blastoderm of Chick with seven pairs of Mesoblastic Somites.

that the primitive streak is converted directly into a part of the embryo, that is to say, the part of the embryo posterior to, and including the first pair of mesoblastic somites.

With regard to the area vasculosa, my experiments seem to indicate that the part of the blastoderm which becomes area vasculosa is that part which lies on the inner edge of the posterior part of the area opaca of the unincubated blastoderm. It is along this edge where, according to Koller,* a white crescent is always visible.

Koller, further, asserts that this white crescent is grooved. From this crescent and groove Koller derives the primitive streak and primitive groove by the conversion of the transverse crescent and groove into a longitudinal streak and groove.

I think that all recent authors are agreed that it is not grooved, and most admit that it has nothing to do with the primitive streak.

It is, however, quite true that a crescentic whiter area is sometimes visible here, but in, I think, the majority of cases there is nothing of the kind to be seen.

When it is present a sagittal section of the hinder part of the blastoderm seems to reveal its nature. In such a section a mass of inner-layer cells, which would, perhaps, be more properly described as a band of yolk containing numerous nuclei, although quite sharply marked off from the underlying yolk-mass, can be detected. This area corresponds in position to that part of the blastoderm from which, according to experiments made with bristles, the area vasculosa is derived (figs. D, E).

A sable hair inserted in the yolk beyond the limits of the blasto-

* "Beiträge zur Kenntniss des Hühnerkeims im Beginne der Bebrütung," 'Sitz. Akad. der Wissensch.,' Wien, vol. 80, 1879. "Untersuch. über d. Blätterbildung im Hühnerkeim," 'Arch. f. mikr.-Anat.,' vol. 20, 1881.

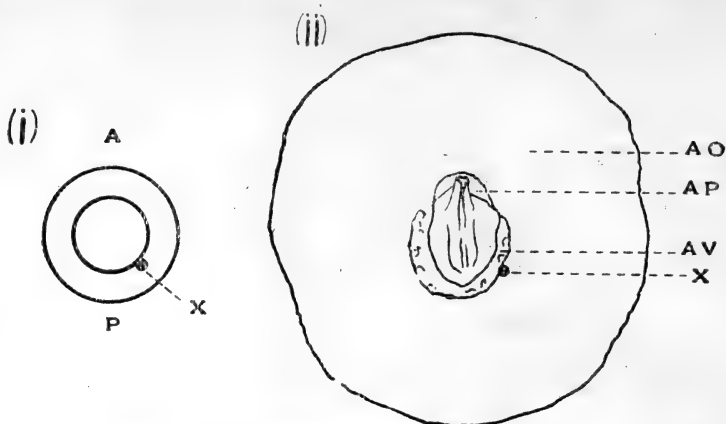


FIG. D.—(i) Diagram of unincubated Blastoderm.
(ii) Blastoderm after 24 hours' incubation.

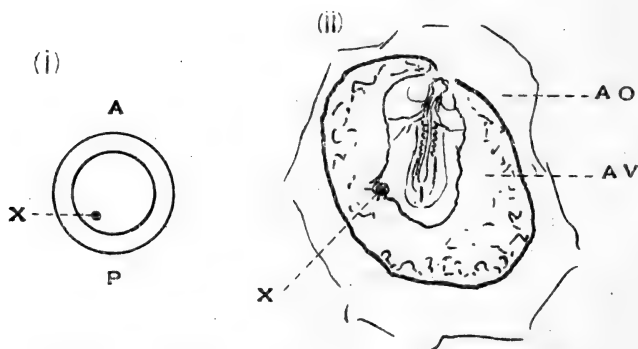


FIG. E.—(i) Diagram of unincubated Blastoderm.
(ii) Blastoderm with five Pairs of Mesoblastic Somites.

derm, if placed close to the edge of the blastoderm generally hinders the development of that side. But if placed at some distance from the blastoderm, it is eventually passed by the advancing edge of the blastoderm, and is found within the area opaca, though usually a streak is left between it and the edge of the blastoderm.

December 17, 1896.

Sir JOSEPH LISTER, Bart., F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Right Hon. Sir John Eldon Gorst, a member of Her Majesty's Most Honourable Privy Council, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

- I. "On the Dielectric Constant of Liquid Oxygen and Liquid Air."
By J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London, and JAMES DEWAR, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, &c.
- II. "On the Effect of Pressure in the surrounding Gas on the Temperature of the Crater of an Electric Arc. Correction of Results in former Paper." By W. E. WILSON, F.R.S., and G. F. FITZGERALD, F.R.S.
- III. "Influence of Alterations of Temperature upon the Electrotonic Currents of Medullated Nerve." By AUGUSTUS W. WALLER, M.D., F.R.S.
- IV. "On Subjective Colour Phenomena attending sudden Changes of Illumination." By SHELFORD BIDWELL, M.A., LL.B., F.R.S.
- V. "On the Occurrence of Gallium in the Clay-ironstone of the Cleveland District of Yorkshire." By W. N. HARTLEY, F.R.S., and H. RAMAGE.
- VI. "On some Recent Investigations in connection with the Electrodeposition of Metals." By J. C. GRAHAM.

The Society adjourned over the Christmas Recess to Thursday, January 21, 1897.

FRSL 60

“On the Dielectric Constant of Liquid Oxygen and Liquid Air.” By J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London, and JAMES DEWAR, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, &c. Received December 8,—Read December 17, 1896.

The exceedingly high insulating properties of liquid oxygen and liquid air indicate that these bodies are dielectrics, and possess a dielectric constant or specific inductive capacity which it is necessary to determine. We have, therefore, lately made some measurements which have enabled us to assign a number representing, in all probability, a close approximation to these constants.

The remarkable non-conducting quality of these liquid gases for electricity enabled us to employ a method which, generally speaking, is not applicable to liquids other than those of very high specific resistance, or insulating power.

The method used by us in these experiments consists in the employment of a small condenser composed of metal plates which can be plunged beneath the surface of the liquid gas, and the capacity of this condenser measured when the dielectric between the plates is first gaseous air at ordinary temperature and pressure, and is next replaced by the liquid oxygen or liquid air. In order to determine the capacity of this condenser, which is necessarily small and of the order of 0·001 microfarad, we adopted the well-known device of charging the small condenser with a high potential (100 volts) and then discharging it into a much larger, well insulated mica condenser, having a capacity of about 0·5 microfarad. This process was repeated ten times, and the larger condenser was then discharged through a standardised ballistic galvanometer. A specially constructed and highly insulated key was employed to charge the small condenser by means of a battery of fifty small lithanode secondary cells; and then to discharge it into the larger condenser. The success of this method depends entirely on the absence of sensible leakage in the condensers, and it is essential to show that the small condenser loses no sensible portion of its charge by leakage or conduction during the interval which elapses between disconnecting it from the battery and connecting it to the large condenser, which acts as a reservoir.

In these experiments the small condenser consisted of seventeen plates of carefully flattened aluminium, about 1 mm. in thickness; each plate being 5 cm. wide by 15 cm. long. In order to separate the plates, small distance pieces of crown glass were employed,

each fragment being about 3 mm. square and 1 mm. thick. Four of these fragments were affixed to each metal plate with a touch of shellac at the four corners and one fixed in the middle. The seventeen plates were then piled one on the other, the glass fragments acting as separators, and the alternate plates were connected together by wires soldered to each series. A metal clamp kept all the plates in position. The condenser so formed consisted of seventeen plates, eight being the positive, and nine the negative surfaces. The glass distance pieces had a total surface of very nearly 1 per cent. of the total opposed surface of the plates. The condenser so formed had a capacity of 0.001031 of a microfarad when gaseous air at 15° C. and normal pressure formed the dielectric.

If such a condenser having a capacity C' is charged to a potential V and then discharged n times in succession into a larger reservoir condenser of capacity C , it is easy to show that at the end of the n successive charges the quantity Q contained in the large condenser is given by the series

$$Q = C'V(m + m^2 + m^3 \dots m^n),$$

where

$$m = \frac{C}{C + C'}.$$

Hence we have

$$Q = C'V \frac{m}{1-m} (1-m^n).$$

The capacity C' of the small aluminium condenser may be considered to be made up of two parts; a part which is changed when liquid oxygen is substituted for gaseous oxygen or air on immersing the condenser, and which thereby becomes increased. If K is the dielectric constant of liquid oxygen, referred to that of gaseous oxygen at -182° C. as unity; and if c is the capacity of this variable part of the condenser when the dielectric is gaseous oxygen, then Kc is its capacity when liquid oxygen is substituted for the gaseous oxygen at the same temperature.

In the next place there is a small part of the whole capacity due to the glass separators. These, as a whole, have a surface very nearly equal to 1 per cent. of the whole surface of the metal plates, and a dielectric constant, as shown below, when cooled to -182° C., of 5.0. Hence it follows that that part of the whole capacity of the condenser which is due to the glass separators, may be represented very nearly by $5c/100$.

This part of the capacity remains practically constant whether the condenser is lifted out of the liquid oxygen into the cold gaseous oxygen lying above it, and which is at nearly the same temperature, or put into it, as long as the condenser is very nearly at the same temperature in the two conditions.

Hence, when the small condenser is under the surface of liquid oxygen its capacity C' , as a whole, is

$$Kc + 0.05c,$$

and the whole quantity of electricity, Q , given up to the reservoir condenser after n charges of the small one, charged to potential V , have been put into it, is

$$Q = Vc(K + 0.05) \frac{m}{1-m} (1-m^n) \\ = Vc(K + 0.05)M,$$

where $m = \frac{C}{C + (K + 0.05)c}$ and $M = \frac{m}{1-m} (1-m^n)$.

Again, when the small condenser is lifted out of the liquid oxygen into the gaseous oxygen lying on the surface, its capacity becomes $c + 0.05c = 1.05c$, and the whole quantity Q' stored up in the reservoir condenser, after n charges at a potential V , is

$$Q' = Vc(1.05) \frac{m'}{1-m'} (1-m'^n) \\ = Vc(1.05)M',$$

where $m' = \frac{C}{C + 1.05c}$ and $M' = \frac{m'}{1-m'} (1-m'^n)$.

If in each case the reservoir condenser is discharged through a ballistic galvanometer, the "throw" or elongation of which is proportional to the quantity of electricity sent through it, and if θ and θ' are the throws produced by the quantities Q and Q' , we have

$$\frac{\theta}{\theta'} = \frac{Q}{Q'} = \frac{K + 0.05}{1.05} \frac{M}{M'}.$$

The ratio θ/θ' is given from the observations.

To solve this equation completely and determine K would be difficult, since the quantity M is a somewhat complicated function of K .

We know, however, that the ratio of M/M' cannot be very far from unity. A rough experiment had shown that K was a number in the neighbourhood of 1.5, and a calculation shows that when ten discharges of the small condenser are made in each case into the large condenser, and if the large condenser has a capacity of 0.5 microfarad, and the small one a capacity of nearly 0.001 microfarad, that the ratio $M/M' = 1030/1019$ nearly. Hence M/M' comes in as a correcting factor of about 1 per cent. in value.

Before relying on the above method, it was necessary to prove that the loss of charge of the small condenser was negligible during the time elapsing between the end of the charge and the end of the discharge of the small condenser.

We found on trial that although the small condenser had a capacity of only 0.001031 microfarad, it held its charge when charged with 100 volts, and placed beneath the surface of liquid air in the most extraordinary way. The test for insulation was as follows:—

The small condenser was charged with 100 volts, and discharged through the galvanometer instantly. The galvanometer throw was 95 scale divisions.

The small condenser was then charged and allowed to stand ten minutes insulated. It was then discharged through the galvanometer, and the throw was 90 scale divisions. In like manner it was charged and insulated for forty-seven minutes, and the throw was then 80 scale divisions.

The above figures show that the charge of the small insulated condenser decreased only by about 15 per cent. in three-quarters of an hour when placed beneath liquid air, and hence the loss of charge in one-tenth of a second was quite inappreciable.*

The same remarkable insulation is found when the small condenser is held in the cold gaseous oxygen lying above the liquid oxygen. The low temperature of -182°C . prevents any sensible leakage across the glass distance pieces, and also increases the specific resistance of the glass itself.

As a further instance of the very high insulating power of liquid air, we may mention that we charged the small condenser when immersed in liquid air with a Wimshurst electrical machine, and, after insulating the condenser and waiting a few moments, closed the terminals of the condenser by a wire. A small spark was seen at the contacts. We thus constructed a little Leyden jar, the dielectric of which was liquid air, and the coatings the aluminium plates. This liquid Leyden jar held its charge perfectly.

Having satisfied ourselves in this manner that the condenser when immersed in liquid air would lose no sensible portion of its charge during the fraction (about one-tenth) of a second in which the charge and discharge key was moving between its contacts, we proceeded to experiment in the following manner. The condenser was placed in a very large vacuum vessel, holding about two litres of liquid oxygen, and it was charged as described, and discharged into a very good mica condenser, made by Dr. Muirhead, which had an exceedingly high insulation. The process of charging and discharging ten times occupied, perhaps, two seconds.

* These figures do not of course measure the electrical resistance of the liquid oxygen alone. They show, however, that the immersion of the condenser in liquid oxygen enormously decreased or entirely destroyed any surface leakage over the small glass separators, and, as we have found by an independent examination, increased the resistivity of the glass itself. The specific resistance of liquid oxygen itself is exceedingly high.

The resultant charge having been measured on the ballistic galvanometer, the condenser was lifted out into the cold gaseous oxygen lying on the surface of the liquid oxygen, and before the condenser had time to alter its form by rising in temperature, the same process was repeated with the dielectric changed to gaseous oxygen at -182°C .

The following Table I shows the observed ballistic throws, all reduced to their equivalents at one common charging pressure of 100 volts :—

Table I.—Observations to Determine the Dielectric Constant of Liquid Oxygen.

	Potential to which the condenser was charged in volts.	Ballistic throw in cm., corresponding to 10 charges of the small condenser.	Ballistic throw, reduced to correspond to 10 charges of the small condenser at 100 volts.
Exp. I.—Condenser at ordinary temperature, 15°C .	103·3	7·7	7·45
	103·2	7·75	7·51
	103·2	7·75	7·51
Exp. II.—Condenser in liquid oxygen at -182°C .	103·15	11·3	10·96
	103·1	11·25	10·91
	103·1	11·27	10·93
	103·0	11·27	10·94
Exp. III.—Condenser in cold oxygen gas above the liquid oxygen at -182°C .	101·3	7·65	7·55
	101·2	7·6	7·51
	101·2	7·6	7·51
	101·2	7·58	7·49
	101·2	7·56	7·51
Exp. IV.—Condenser in liquid oxygen.	101·3	10·9	10·77
	101·2	10·85	10·72 } bad.
Exp. V.—Condenser in cold oxygen gas above the liquid oxygen.	101·3	7·60	7·50
	101·3	7·60	7·50
	101·3	7·58	7·48
	101·3	7·57	7·47
Exp. VI.—Condenser in liquid oxygen.	101·4	11·1	10·95
	101·3	11·0	10·86
	101·3	10·95	10·81
	101·3	11·0	10·86

Mean ballistic throw in gaseous oxygen = $7·502 = \theta'$. Mean ballistic throw in liquid oxygen = $10·903 = \theta$.

It will be seen that the mean galvanometer throw, when the condenser was immersed in liquid oxygen, was 10·903 centims., and the mean throw, when raised into the gaseous oxygen, was 7·146 centims.

One matter which we felt it important to examine, was whether there was any correction needed for the change in the dielectric constant of the glass separators with temperature.

Since these glass separators had a total surface of nearly 1 per cent. of the area of the metal plates, the condenser may be regarded as consisting of two condensers joined in parallel, one consisting of a glass dielectric condenser having an effective surface of 1, and the other a condenser having a liquid or gaseous oxygen dielectric having an effective area of 99. In the course of these experiments we have therefore examined the effect of low temperature upon the dielectric constants of glass, paraffined paper, and mica. We find that on cooling these bodies to -182°C . they experience a marked reduction in dielectric constant. The dielectric constant of a certain specimen of crown glass was reduced by 21·4 per cent. by cooling to the temperature of liquid air or to -185°C . The dielectric constant of paraffined paper was reduced by 28·4 per cent. under the same circumstances.* We are engaged in a systematic examination of the influence of very low temperatures on the dielectric constants and specific resistances of the principal dielectric bodies. The crown glass used as separators in the construction of our small condenser had a specific inductive capacity of about 6·0 at ordinary temperature, and this at the low temperature would be reduced to nearly 5·0. Hence in estimating the capacity of the condenser, as constructed, there comes in as we have seen a correction from the presence of the glass. We selected glass in the first instance rather than ebonite or sulphur, as we thought it probable we should use the same condenser in determining other dielectric constants, and we wished to construct the separators of a material which was very rigid and not easily acted upon by oils or other liquids.

Taking the formula above given, we can deduce from the observed results the required constant, for, we have

$$\frac{\theta}{\theta'} = \frac{K + 0\cdot05}{1\cdot05} \frac{1030}{1019},$$

and hence substituting for $\frac{\theta}{\theta'}$ the observed ratio $\frac{10\cdot903}{7\cdot502}$, we find

$$K = 1\cdot491$$

* By another method we have found that for the glass of a glass test-tube the dielectric constant was decreased 22·2 per cent. by cooling to the temperature of liquid air. Under the same circumstances a certain specimen of mica decreased only 3·01 per cent. in dielectric constant.

as the dielectric constant of liquid oxygen referred to that of the overlying gaseous oxygen at -182° C. as unity. Since the aluminium condenser is at the same temperature when the two measurements are made, no correction is necessary for any change of form of the condenser.

To determine the dielectric constant of liquid oxygen in terms of that of a vacuum taken as unity, we require to know the dielectric constant of the gaseous oxygen lying on the surface of the liquid oxygen referred to the same unit.

Boltzmann and Klemencic have both shown that the true dielectric constant of air at a temperature of 0° C. and 760 mm. is 1.00059. That of oxygen gas at the same temperature and pressure is not very different. If the value of $K-1$ for gases varies directly as the pressure, and if temperature *per se* makes no difference, then the dielectric constant of the gaseous oxygen lying on the surface of the liquid oxygen, and which has a temperature of -182° C. nearly, and a density about three times that of the gas at 15° C., is not far from 1.002. Hence the correcting factor to be applied to the above value of the dielectric constant of the liquid is at the most 1.002, and the true dielectric constant of liquid oxygen at -182° C. and under a pressure of 760 mm. is not far from 1.493.

We intend to examine this correction more closely.

As a matter of fact, we were not able to detect any difference between the capacity of the small condenser when held in air at ordinary temperature (15° C.) and pressure, and in the cold gaseous oxygen at -182° C. lying on the surface of the liquid oxygen.

Until we are able to make a better determination we may take the above number, 1.491, therefore, as representing in all probability a close approximation to the dielectric constant of liquid oxygen.

The interesting question then arises how far does liquid oxygen obey Maxwell's law, by which the product of the dielectric constant and the magnetic permeability should be equal to the square of the refractive index for waves of infinite wave-length? The materials are at hand for making this comparison, as we have ourselves just determined the magnetic permeability of liquid oxygen, and find it to be 1.00287,* and the refractive index of liquid oxygen has been determined by Professors Liveing and Dewar for several different wave-lengths.†

* See Fleming and Dewar, 'Roy. Soc. Proc.,' December, 1896, vol. 60, p. 283, "On the Magnetic Permeability of Liquid Oxygen and Liquid Air."

† Liveing and Dewar, 'Phil. Mag.,' Sept., 1895, p. 269, "On the Refraction and Dispersion of Liquid Oxygen and the Absorption Spectrum of Liquid Air." See also Liveing and Dewar "On the Refractive Index of Liquid Oxygen," 'Phil. Mag.,' August, 1892, "On the Spectrum of Liquid Oxygen and on the Refractive Indices of Liquid Nitrous Oxide and Ethylene;" also Liveing and Dewar, 'Phil.

Professors Liveing and Dewar determined the refractive indices (μ) corresponding to certain wave-lengths (λ) for the following wave-lengths:—

From lines in the spectrum of	λ .	μ .
Cadmium	4416	corresponds to 1.2249
	6438	„ 1.2211
Thallium.....	5350	„ 1.2219
Lithium.....	6705	„ 1.2210
Sodium	5892	„ 1.2114

They state that they consider the best results are given by the first two observations. Taking these wave-lengths 4416 and 6438, and the refractive indices corresponding to them, we have calculated from them, by the formula

$$\mu_{\infty} = \frac{\mu\lambda^2 - \mu_1\lambda_1^2}{\lambda^2 - \lambda_1^2},$$

the refractive index for infinite wave-length (μ_{∞}) and found it to be as follows:—

$$\mu_{\infty} = 1.2181.$$

The square of this number is 1.4837, and this, therefore, is the value of the square of the refractive index for waves of infinite wave-length in liquid oxygen.

Taking the product of the dielectric constant, $K = 1.491$, as above determined, and that of the magnetic permeability, $p = 1.00287$, as previously obtained by us, we find that this product Kp is 1.495, and hence that there is therefore a very fairly close agreement between the number representing the square of the refractive index for waves of infinite wave-length and the above product. The difference amounts to about two-thirds of one per cent. Hence liquid oxygen is a substance which very closely obeys Maxwell's law.

We have applied the same apparatus to the determination of the dielectric constant of liquid air obtained in exactly the same manner, and Table II below gives the results of the observations taken in liquid air. The observed results, when corrected as above described, give for the dielectric constant of liquid air the number 1.495, which is slightly more than that of the liquid oxygen. As, however, by the time the experiment was complete the liquid air had practically become liquid oxygen owing to the nearly complete evaporation of the nitrogen, the coincidence of the two results is only what was to be expected.

Mag.,' October, 1893, "On the Refractive Indices of Liquid Nitrogen and Air," also Liveing and Dewar, 'Phil. Mag.,' Sept., 1888, "On the Absorption Spectrum (luminous and ultra-violet) of large Masses of Oxygen."

The Table II below gives the observational results in the case of the liquid air—really, however, of liquid oxygen.

Table II.—Dielectric Constant of Liquid Air (practically Liquid Oxygen).

In Liquid Air. Ballistic throw for condenser charged to 100 volts.	In cold Gaseous Air. Ballistic throw for condenser charged to 100 volts.
9.5	—
9.6	—
9.5	—
—	6.5
—	6.6
—	6.6
9.4	—
9.5	—
9.55	—
—	6.51
—	6.51
9.7	—
9.55	—
Mean = 9.54	Mean = 6.54

Dielectric constant = 1.495.

With regard to the above-determined dielectric constants for liquid oxygen and liquid air, it may be remarked that these numbers are smaller than those which have been obtained for almost any other solid or liquid substance of which we have been able to find the measured results. It has been already pointed out that large dielectric constant generally accompanies small specific resistance in a dielectric, and *vice versa*. Hence, as the specific resistance of the liquid oxygen is very large—it being a very fine insulator—it is not surprising to find the dielectric constant very small. As above mentioned, at a very low temperature, the dielectric constant of some other solid dielectrics has been found by us to be very much reduced, and hence an interesting field of research is opened out for the examination of the change produced by low temperatures on the dielectric constants of other well-known solid insulators, such as paraffin, ebonite, gutta-percha, mica, sulphur, spermaceti, and various frozen liquid insulators, such as the numerous hydrocarbon oils, carbon disulphide, ice, &c.* We hope to

* Mr. W. Cassie, M.A., 'Phil. Trans.,' vol. 46, 1889, has given the results of measurements on the changes produced in the dielectric constants of various insulators by heating them. As far as we can see, our initial results at low temperatures for glass and paraffin are consistent with his. It will be interesting to see how this relatively small dielectric constant of liquid oxygen compares with that of other dielectrics when these last are cooled to the same temperature.

be in a position shortly to furnish further information on this point, and, also, if possible, to say whether the fall in dielectric constant is accompanied by a reduction in the refractive index; that is to say, whether Maxwell's law is obeyed at low temperatures.

We may add that we have already devised a method by which it will be possible to construct a condenser without the above-described distance pieces, and hence to free the resulting measurement from the small uncertainty—amounting, perhaps, to about 1 per cent.—which may affect the above-given numerical results, and which comes in in consequence of the doubt existing as to the exact area of the separators, and also the exact dielectric constant of the glass at the low temperature.

It is interesting to observe that the numbers which we have found above for the dielectric constant of liquid oxygen and liquid air are not very different in order, though somewhat smaller than the dielectric constant as already determined for some other liquid gases,* such as nitrous oxide and carbon dioxide.

In conclusion, we may add that we have been again much indebted to Mr. J. E. Petavel for his kind assistance in making the above-described observations and measurements.

Note added December 15.

In connection with the above investigation, it is interesting to note one remarkable difference between the magnetic susceptibility of oxygen in the liquid and in the gaseous state. The mass of 1 c.c. of gaseous oxygen, taken at 15° C. and 760 mm., is 0.00134 gramme. The mass of 1 c.c. of liquid oxygen, taken at -182° C. and 760 mm., as determined by one of us (J. Dewar), is 1.1375 gramme. Hence the ratio of the density of liquid oxygen to that of gaseous oxygen is 849 to 1.

The magnetic susceptibility of gaseous oxygen at 15° C. and 760 mm., as obtained from the figures given by Faraday and E. Becquerel, is 0.143×10^{-6} per unit of volume, whilst the magnetic susceptibility in the liquid state is, as we have shown,† 228×10^{-6} . Hence the ratio of the magnetic susceptibility of liquid oxygen to that of gaseous oxygen for equal volumes is 1594 to 1.

In other words, the magnetic susceptibility of liquid oxygen is nearly twice as great as that of gaseous oxygen for equal masses. The inference is that magnetic susceptibility is not merely a property of the molecule *per se*, but is a function of the state of aggregation.

* See F. Linde, 'Journal de Physique,' vol. 5, Sept., 1896, p. 413, "On the Dielectric Constant of Liquid Gases."

† See Fleming and Dewar, 'Roy. Soc. Proc.,' vol. 60, p. 283, December, 1896.

Note added December 18.

In addition to the arrangements above described for determining the capacity of the small condenser, we have also employed the well-known method of charging and discharging the small condenser through a galvanometer by means of a contact-maker driven at a speed of sixty contacts per second by an electrically controlled tuning-fork. By this means a steady deflection of the galvanometer is obtained due to the passage of the rapidly recurring discharges through it. Preliminary observations with this apparatus have confirmed the above-given value for the dielectric constant of liquid oxygen, and by a modification of it we hope shortly to make a very careful re-determination of the constant.

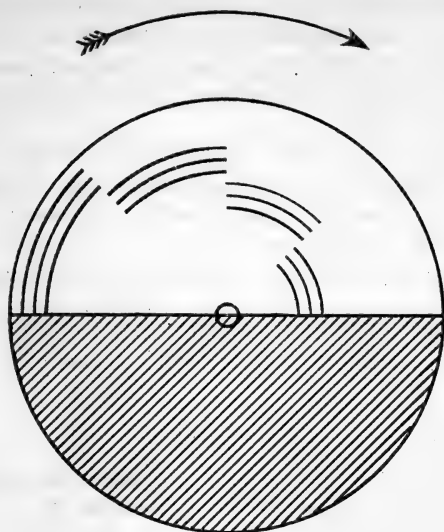
“On Subjective Colour Phenomena attending sudden Changes of Illumination.” By SHELFORD BIDWELL, M.A., LL.B., F.R.S. Received December 10,—Read December 17, 1896.

The investigation which forms the subject of this paper originated in an attempt to account satisfactorily for the colour phenomena exhibited by Mr. C. E. Benham's “Artificial Spectrum Top,” which, when it was brought before the public, about two years ago, excited considerable interest.

The top consists of a disk of cardboard about $4\frac{1}{4}$ in. (10·8 cm.) in diameter, mounted upon a spindle. One half of the disk is painted black; upon the white ground of the other half are drawn four successive groups of three black lines, having the form of concentric arcs of 45° , which are at different distances from the centre, as shown in the annexed figure; the thickness of the lines is about $\frac{1}{8}$ in. (1 mm.). When the disk rotates, each group of black lines generally appears to assume a different colour.

The nature of the colours thus developed depends upon the speed of the rotation, and upon the quality and intensity of the illumination. After several trials, I found that no better results, on the whole, could be obtained than when the disk was illuminated by a 16-candle power incandescent lamp, with a ground glass bulb, at a distance of about 6 in. (15 cm.), and was caused to turn about five times in a second. These, therefore, were adopted as the standard conditions for my experiments, the disk being mounted upon a horizontal axis, driven by an electro-motor, and the speed regulated by comparison with the ticks of an ordinary watch.

When the disk rotates under the specified conditions and in the direction indicated by the arrow in the figure, the inner group of



Benham's Top.

lines appears, to my vision, to become bright red, the next group pinkish-brown, the next a dilute olive-green, and the outer group dark blue. If the direction of rotation is reversed, the order of the colours is also reversed.

By far the most striking of these several hues is the first named; hardly any one has the slightest hesitation in pronouncing it to be bright red. As to the blue, there is very rarely any difference of opinion, though it has sometimes been called bluish-green. The hues of the two intermediate groups are much more undecided and difficult to specify, especially when they are seen separately.

The only serious attempts that I know of to explain the origin of the colours shown by the top are those of Professor Liveing and of Captain Abney.* Professor Liveing's explanation is based upon the two hypotheses that the eye perceives certain of the coloured constituents of white light more quickly than others, red being the first to show itself, and that the duration of the impressions due to the different constituents also differs, blue being the last to disappear. Captain Abney thinks that the results would be sufficiently accounted for if the order of persistence of the three colour sensations were violet, green, and red.

Several objections might be urged against these explanations, but the adequacy of either of them seems to be conclusively negated by

* 'Nature,' vol. 51, pp. 167, 292.

the fact that if the thickness of the lines on the disk is much greater than 1 mm., or, more accurately, if it subtends at the eye a greater angle than about one-fifth of a degree, the red and some of the other colours appear only upon the borders of the lines, their inner portions remaining black or grey.

The true solution, at least as regards the red and the blue, is, I think, to be looked for in certain phenomena attending sudden changes of illumination, which, so far as I have been able to ascertain, have not hitherto been observed.

The following are a few out of a large number of experiments that have been made during the last four months. They are described, as far as possible, in logical and not in chronological order. Persons unaccustomed to visual observations will not easily perceive some of the effects mentioned.

Experiment I.

A circular aperture $\frac{1}{2}$ in. (1.3 cm.) in diameter was made in a sheet of blackened zinc and was covered with thin white writing paper. Diametrically across the aperture a strip of tinfoil $\frac{1}{25}$ in. (1 mm.) wide was attached to the paper. The aperture was closed by a shutter, which could be very rapidly opened by means of a strong spring. The sheet of metal was placed over a window in one side of a light-tight box, inside which at a distance of 1 ft. (30 cm.) from the aperture was an incandescent lamp of 8-candle power with a ground glass bulb. The observations were made at a distance of about 1 ft. from the box, the room being in darkness.

When the shutter was suddenly opened, several curious phenomena appeared simultaneously. The period of their duration was difficult to estimate; it was probably more than one-twentieth of a second and less than one-tenth.

(1) Immediately after it was revealed, the small luminous disk first increased in size with extreme rapidity, and afterwards became somewhat smaller, being in its final condition still larger than at the moment of exposure. This effect was more easily seen when the tinfoil strip was looked at: it seemed to become at first much thinner, then thicker again.

(2) At the moment when the disk was uncovered, a luminous halo, like a broad ring, appeared to start from its margin and spread outwards through a distance of more than an inch (2.5 cm.) in every direction; then it rapidly contracted and disappeared. The halo was blue or blue-violet in colour, and seemed more sharply defined upon its inner than upon its outer border.

(3) Contemporaneously with the existence of the halo, the disk was surrounded by a bright red corona, which, like the halo, expanded outwards, and then contracted. There was not, however,

at any stage a dark interval between the corona and the disk; more probably the inner edge of the corona was slightly within the apparent permanent boundary of the disk. The red corona was very narrow; its greatest width appeared to be rather less than 1 mm., or about one-fifth of a degree. The effect was best seen when the attention was directed upon the tinfoil strip, which for a moment, after the exposure, became bright red, the coronæ, or red borders, of the adjoining semi-disks meeting or perhaps overlapping one another. The apparent temporary excess of the area of the disk above its final area, as mentioned in (1), was probably due to the evanescent red border.*

It is remarkable that repeated experiments had been made with this and similar apparatus for several weeks before the existence of the red border was detected, even though something of the kind was looked for. The difficulty is, not to see it, but to know that one sees it; when once it has been perceived it becomes very conspicuous. The phenomenon is beyond doubt constantly met with, and habitually ignored, in daily life. Since my first observation of it I have many times noticed flashes of red upon the black letters of a book or upon the edges of the page; bright metallic or polished objects often show it when they pass across the field of vision in consequence of a movement of the eyes, and it was an accidental observation of this kind that suggested the following experiment.

Experiment II.

(1) The zinc plate of the last experiment was taken from the box, and the aperture in the plate was covered with thin paper. A ground glass lamp of 8-candle power, attached to a flexible cord, was put behind it, and the whole was moved rather quickly either backwards and forwards or round and round in a small circle at a distance of a foot or so from the eyes. The edges of the straight or circular streak of light thus formed were bordered with red.

(2) A 16-candle power lamp was substituted for the other. The red border then appeared to have a greenish-blue band inside it, slightly encroaching upon the streak of light; probably, however, it was only the apparent or irradiation boundary that was thus affected, not the true geometrical boundary.

(3) The paper was removed, and the 8-candle power ground-glass lamp was again placed behind the aperture. The red could now no longer be seen, but the greenish-blue border remained.

(4) When the 16-candle power lamp was used in the same way

* The effect may be seen without the use of the spring shutter, if a black screen be held before the eyes and suddenly removed, but it is more difficult to hit upon the exact position of the disk.

without any intervening paper, no coloured border could be seen, owing, as it seemed, to the glare.

Experiment III.

The aperture in the metal plate was again covered with white paper, having a strip of tinfoil across it, and the plate was fixed before the window in the box, as in Experiment I; a 16-candle power lamp was placed immediately behind it. When the lamp was switched on, the red border was distinctly seen to be backed with greenish-blue, the red itself being much less evident than when the lamp was 18 in. (45 cm.) behind the aperture.

I have hitherto failed to detect any greenish-blue near the border when the disk was suddenly illuminated by the shutter method of Experiment I, instead of by switching on the lamp.*

Experiment IV.

The object of this experiment was to ascertain whether the red border could be produced by the sudden accession of light which contained no red constituent. Ten different coloured glasses were successively interposed between the lamp and the aperture with the shutter. In every case when the spectroscope showed that the glass transmitted red light, the tinfoil strip became red, but never otherwise. For example, it reddened with a dark blue cobalt glass, but not with a blue glass which transmitted much more light, but intercepted the red end of the spectrum.

Experiment V.

The momentary redness around the edge of the suddenly illuminated disk and along the tinfoil strip, as described in the account of the previous experiments, can only be seen by a practised observer. By a different method, however, it can be made quite evident to almost any person whose vision is normal.

The paper-covered aperture in the box was arranged as before, but the shutter was not used. An incandescent lamp was placed inside the box, and a second lamp outside, at a distance of a few inches from the aperture, the observer's eyes being shaded from it by a screen. The tinfoil strip was on the interior side of the paper, and nothing was seen of it from outside, except when the lamp in the box was alight.

A rotating commutator was constructed, by means of which

* [Since this was written, I have found that the greenish-blue may be shown by the shutter method without difficulty if the distance of the lamp from the aperture is suitably adjusted.—Dec. 19.]

current could be supplied to the two electric lamps in the following manner:—During half a turn of the commutator, no current to either lamp; during the succeeding one-sixth of a turn, current to the interior lamp only; during the remaining one-third of a complete turn, current to the exterior lamp only.

Starting with darkness, and turning the commutator quickly through 180° , the observer saw, as soon as the interior lamp was lighted, the shadow of the tinfoil, which was, as usual at the initial stage, of a bright red hue; but a small fraction of a second later, before it had time to lose its redness and become black, the image was obliterated by a flood of light from the exterior lamp, while at the same moment the other lamp was extinguished.

When the commutator was caused to make four or five turns per second, the image of the tinfoil was almost continuous, and was at once recognised by inexperienced observers to be red.*

This experiment was repeated in another form, the arrangement being such that the light of two lamps was interrupted by screening, instead of by breaking the current; the changes in the illumination could thus be made more rapidly.

Two black cardboard disks, from each of which a sector of 60° had been cut out, were mounted $3\frac{1}{2}$ in. (9 cm.) apart at the ends of a horizontal axle, being so fixed that the posterior edge of the opening in one of the disks was exactly opposite to the anterior edge of that in the other. Between the disks, and in a parallel plane, was suspended a sheet of white paper, across the middle of which a narrow strip of tinfoil was gummed. Two clear glass electric lamps were placed near the outer faces of the disks at the same height as the axis, the incandescent filaments being directed horizontally. To an observer looking at the plain side of the paper across the edge of one of the disks, while they were rotating slowly in the proper direction, the paper first appeared dark all over, then it was illuminated from behind by one of the lamps, the dark strip becoming visible; finally, it was illuminated from the front by the other lamp, and the strip could no longer be seen. When the angular velocity was sufficiently increased, the strip was seen continuously, or nearly so, and its colour was, as before, bright red.

Experiment VI.

From a disk of white cardboard 6 in. (15 cm.) in diameter a sector of 60° was cut out; the remainder of the disk was divided into two

* The lamps used in this experiment were made to my order. They are of 8-candle power and have very thin filaments, the efficiency being 2.5 watts per c.p. They were worked at a pressure of 6 per cent. above their marked voltage, and the incandescence responded very quickly to the current.

equal parts by a straight line from the centre to the circumference, and one of these parts was painted black. The disk was attached to a horizontal spindle, turned by a motor at the rate of five or six revolutions per second, while its front was illuminated by a lamp of 16-candle power. A white card, upon which was a black line, or a design composed of black lines, was supported behind the disk, and viewed intermittently through the open sector. When the rotation was such that the open sector succeeded the black portion of the disk and was succeeded by the white portion, the black lines became red.

This experiment is identical with the last, except that the white ground is illuminated entirely by reflected light. In conjunction with the others, it indicates with certainty the origin of the remarkable red colour shown by Benham's top.

The disk with the open sector affords a much more convenient means than the top of exhibiting the colour phenomena. If a disk with an open sector of 45° or 60° is made of white cardboard, and a movable black half disk is mounted in front of it upon the same axis, we may, by suitably adjusting the position of the black half disk with regard to the opening, produce in a fixed object all the tints shown by the top, as well as intermediate ones; and the object itself may be easily changed to suit the conditions of an experiment.

Experiment VII.

If the commutator of Experiment V, or the disk with the open sector of Experiment VI, be turned in the reverse direction, the strips of tinfoil or the black lines appear to become blue (instead of red), like the outer group of lines in Benham's top when it spins in the direction indicated by the arrow in the figure. This appearance is partly, if not altogether, illusory. It is the bright ground in the immediate neighbourhood of the black lines that becomes blue; the lines themselves (except possibly just within their extreme edges) become a neutral grey, owing to the alternations of light and darkness or of white and black.

A card with some black lines 1 mm. thick drawn upon it was placed behind the disk with the open sector of Experiment VI, which was turned in the direction such that the open sector was preceded by white and followed by black. The lines presented the appearance of having been drawn with blue ink upon imperfectly sized paper, a blue stain having apparently spread for a short distance on both sides of the lines.

Lines of gradually increased thickness were successively employed until at last they had the form of bands $\frac{3}{8}$ -in. wide; and even in this latter case it was not easy to see that the bands themselves did not become blue, but only their outlying borders.

When, however, a visiting card which had been blackened over its whole surface was placed behind the rotating disk, it merely turned a lighter black, or rather grey, in which it was impossible to imagine the slightest tinge of blue.

A small piece of white paper which was subsequently attached to the middle of the card became blue around its edges when the disk was turned, but the blue did not encroach at all (or if at all, only to a very small extent) upon the black ground.

When these observations have been made it becomes possible to recognise that the apparently blue lines in the top are themselves really grey, and only bordered externally with blue.

Experiment VIII.

The natural conclusion from the observations described above is that if a black disk were suddenly formed upon a bright ground, the disk would for a moment appear to be surrounded by a blue border. I was not successful in devising a satisfactory arrangement for suddenly creating a black disk, but the effect is sufficiently shown in the following manner.

An aperture $1\frac{1}{4}$ in. (3 cm.) in diameter was cut in one side of a wooden box and was covered with white paper; one half of the aperture could be suddenly covered by a sliding metal shutter which was actuated by a spring: a lamp was placed inside the box. When the shutter was operated, a blue band 1 or 2 mm. wide appeared on the bright ground just beyond and adjoining the edge of the shutter when at rest. Its duration was thought to be slightly longer than that of the red border of other experiments, and it apparently disappeared by retreating into the black edge of the shutter.

When the shutter was moved by hand across the field at a slower speed, its edge was seen to be preceded by a thin blue border, which, when the shutter reached its limiting stop, appeared to reverse the direction of its motion and return into the shutter.

The blue border is much less conspicuous and more difficult of observation than the red one. In order to see it plainly careful adjustment of the light is necessary. An examination of the effect through coloured glasses was attended by uncertain results.

Remarks on the Experiments.

The phenomenon which in the account of Experiment I has been spoken of as a blue halo may be due either to a momentary sympathetic excitement of the nerve fibres of the retina in the neighbourhood of those directly acted upon by the light, or, as I think, less probably, to light scattered by the imperfectly transparent media of the eye. In the latter case its rapid disappearance might be accounted for

partly by the diminished sensibility of the retina after the first moment and partly by the contraction of the iris. The dark interior of the halo, which begins to appear soon after its formation, is probably connected with a class of visual sensations which have been specially studied by M. Aug. Charpentier.* The sensation of luminosity is followed very shortly after its first excitement by a brief, dark reaction, and it is perhaps the momentary revival of the luminosity after this reaction that gives the halo the appearance of retreating into the bright disk.

But whatever the cause of the halo, there can hardly be any doubt that the corona or narrow red border is due to sympathetic excitation. When the red nerve-fibres of the Young-Helmholtz theory are affected by light the intensity of which does not exceed a certain limit, the immediately surrounding red nerve-fibres are for a short period sympathetically affected, while the violet and green are not so, or in a much less degree.

It must be confessed that it is more difficult to offer a reasonably simple explanation of what happens when the intensity of the light exceeds the limit above indicated, and the band of greenish-blue consequently appears in addition to, or in place of, the red border. It is, perhaps, preferable to refrain at present from any speculation on the subject.

When a Benham's top is spun in bright daylight or weak sunshine, it is quite possible to distinguish both the red and the greenish-blue at the same time, the latter encroaching somewhat upon the white ground; its persistence is greater than that of the red, as can easily be seen when the top is turning rather slowly. The greenish-blue appears to be of the hue that is complementary to red, and it is evidently the development of this colour that makes the red so much less conspicuous when the top is illuminated by daylight than when artificial light is employed.

The obvious method of accounting for the formation of the blue border around a patch in a bright field from which light has suddenly been cut off, is to suppose a brief sympathetic reaction in the nerve-fibres adjacent to those from which the exciting stimulus has been withdrawn, this reaction being more marked in the red fibres than in the green and violet, or perhaps occurring in the red fibres only, at least when the light is of the usual intensity. If the red fibres just outside the darkened patch ceased for a moment to respond to the luminous stimulus, in sympathy with those inside the patch, the appearance of a blue border would be produced.

In sunlight I have sometimes found that the lines in Benham's top which ordinarily appear blue, assumed a reddish colour; under

* 'Comptes Rendus,' vol. 113 (1891), p. 147.

strong illumination therefore the sympathetic dark reaction would seem to be least in the case of the red fibres.

Subjective colours of the same class as those shown by Benham's top, but not nearly so conspicuous, have long been known. Helmholtz* mentions that if a rotating disk with black and white sectors is looked at fixedly, each white sector appears to be reddish along its leading border and bluish along its rear border. He also remarks that these colours are more easily seen upon a disk covered with two spiral bands, black and white, of equal breadth. From these and other observations, Helmholtz concludes that when a point of the retina is exposed to rapid alternations of white light and of darkness, causing successive states of increasing and decreasing excitation, the moment of maximum excitation is not the same for all colours. It has, however, been shown above that in analogous cases the red originates in a portion of the retina which has not been exposed to the direct action of light, while the blue originates in a portion where light has not ceased to act. Helmholtz's supposition therefore does not apply—at least to the class of colours at present under consideration.

I have not made any attempt to account for the more feeble colours exhibited by the two intermediate groups of lines in Benham's top, nor for the changes which occur when the speed of rotation is increased. These effects no doubt result, at least in part, from modifications of the phenomena already discussed. But for the present I am compelled to discontinue the experiments on account of the disagreeable and probably injurious effects which they produce upon the eyes.

“On the Effect of Pressure in the Surrounding Gas on the Temperature of the Crater of an Electric Arc. Correction of Results in former Paper.” By W. E. WILSON, F.R.S., and G. F. FITZGERALD, F.R.S. Received November 30, —Read December 17, 1896.

In May, 1895, a preliminary paper by one of the authors was read at the Royal Society, in which is described the apparatus used for these experiments, and the results which were then obtained.

The primary object of this research was to determine, if possible, whether the temperature of the crater in the positive carbon varies when the pressure in the surrounding gas is changed.

It has been suggested that the temperature of the crater is that of

* ‘Phys. Optik,’ § 23.

boiling carbon. The most modern determinations give this temperature of the crater as about 3300—3500 C.*

If this is the true boiling point of carbon, it is then clear that solar physicists must find some other substance than solid carbon particles to form the photospheric clouds in the sun, as the temperature of this layer is most probably not below 8000 C.,† unless, indeed, the pressure in the solar atmosphere is sufficient to raise the boiling point of carbon to about this temperature (see p. 381). It is in order to throw some light on this subject that these experiments were undertaken.

The gas used in our first experiments was nitrogen, and we found that the radiation from the crater fell off in a most remarkable manner whenever the pressure was raised in the box surrounding the arc. This falling off was not due to any very large extent to visible cloud or smoke, and the crater seemed so much reduced in temperature as to glow with only a red heat. This seemed to show that the temperature of the crater depends on how much it is cooled by the surrounding gas, and not on its being the temperature at which the vapour of carbon has the same pressure as the surrounding atmosphere.

It was found that we were limited to pressures not exceeding about 20 atmos., as at this pressure we could not withdraw the negative carbon sufficiently to see into the crater without the arc breaking. We were then only able to obtain a current from a battery of accumulators which had an E.M.F. of 110 volts. Since then we obtained a Crompton dynamo which could give 300 volts and 15 amperes, and which was driven by a turbine.

From the great difficulty of obtaining a sufficient quantity of pure nitrogen under pressure, we obtained a 20 ft. cylinder of air compressed to 120 atmos. With this we tried a series of experiments, and these at first seemed to corroborate our former ones, in which we used nitrogen, but we found that at any rate some of the radiation, and possibly a great deal of it, was cut off by the formation of what appeared to be red fumes of NO_2 . We found no absorption from this cause so long as the pressure was nearly atmospheric, but at about 100 lbs. pressure this gas was formed with great rapidity, and undoubtedly cut off a great deal of the radiation. We easily confirmed our belief in the presence of this gas by its well known absorption spectrum.

Lest heat dissociation might cause an apparent increase in the amount of NO_2 , we tried heating some of this gas in a flask. We observed that when hot the brown fumes became golden yellow, and

* Wilson and Gray, 'Roy. Soc. Proc.' vol. 58; Violle, 'Journ. de Phys.,' 3rd series, vol. 2, 1893, p. 545.

† Wilson and Gray, 'Phil. Trans.,' A, vol. 185, 1894.

the absorption bands nearly disappeared, so that the heating could not have been the cause of the apparently enormous production of NO_2 at high pressure.

We next tried whether oxygen blown into the arc would burn up the carbons, but found it did not do so to any serious extent, and so tried the arc in a compressed atmosphere of this gas.

The arc burned very nicely indeed in the oxygen, the carbons keeping a good shape, and a very steady crater. The oxygen was, however, so contaminated with nitrogen that at high pressure enormous quantities of NO_2 were again formed, so that we could not proceed further with the radiation experiments. The arc was a bright blue bead, about the size of a pea, and the spectrum was a beautiful banded one.

From these results we concluded that the reduction of radiation, and red-hot appearance of the crater in the former experiments in nitrogen, were due to its being contaminated with oxygen and to the large quantities of NO_2 , which were formed by the arc when under pressure.

We next tried the arc in hydrogen. The gas was obtained as pure, but contained hydrocarbons as an impurity, possibly from having been compressed into a cylinder which had previously been charged with coal-gas.

The arc in hydrogen at atmospheric pressures was a long, thin flame, that moved as far up the carbons as possible; especially on the negative carbon it walked up a cm. along the cone. It went so far that it fused the copper ring that held the negative carbon, and we had to replace it by an iron wire lashing. It was very unsteady, and trees of soot and a deposit of hard graphitic carbon formed on this positive carbon as if there were electrolysis of the hydrocarbon, and carbon were electro-negative compared with hydrogen. This growth took place all round the crater, while there was no tendency for anything to grow on the negative carbon.

The arc was only 5—6 mm. wide, and sometimes over 2 cm. long. There was a green outer flame, with a bright red line not a mm. wide down the middle of it. Where it impinged on the negative carbon there was a bright red flame from the middle of the bright spot on the carbon. The outer greenish part seemed to give much the same spectrum as the green cone in a Bunsen burner, while the red flame and line was undoubtedly glowing hydrogen. As we saw the C and F hydrogen lines very distinctly, the red C line being dazzlingly bright and not nearly so wide as in a coil spark at atmospheric pressure whenever the image of the red part of the arc was thrown on the slit of the spectroscope, the appearance was quite like that of a solar prominence.

The end of the positive carbon was pitted into a number of craters

as the arc was very unsteady, and when the pressure was raised it was almost impossible to keep an arc going, partly because the arc broke when it was elongated the least bit, and partly because a complete lantern of soot trees grew all round the crater, and seemed to short-circuit the arc from time to time.

The arc being very unsteady, no satisfactory reading of the voltage and current was possible. At from 60 to 80 lbs. pressure the voltage varied from 60—80, and the amperes kept continually varying from 15—20. At 40 lbs. with 20 amperes the volts varied from 50—60. The crater was not well developed, so that the radiation observation, even at low pressures, was not very satisfactory, while at high pressures the arc was too short to see into the crater at all, and the lantern of soot trees hid a considerable length, 3 or 4 mm. of the negative carbon besides. The radiomicrometer gave 440 divisions with a good arc in air, and 380 with the moderately good crater in hydrogen. But this difference is no greater than would often occur with a good and moderately good crater, so that there is not any proof of a difference of temperature due to cooling power of hydrogen. These experiments showed us that it was quite hopeless to get any measures of radiation under pressure with hydrogen.

We finally tried an atmosphere of carbon dioxide. We used a cylinder of liquid CO_2 , which was connected to our arc box by a copper tube and stop valve. The arc burned fairly well in this gas, and, except for the difficulty of getting a sufficiently long arc at pressures above 150 lbs., some pretty satisfactory measures of radiation were obtained. We found that whenever the pressure was suddenly reduced, there was a fog formed in the box, which cut off the light enormously. Also by looking down the steel tube, which is closed at its end by a lens, we could see powerful convection currents in the gas which scattered a lot of light. At high pressure the refraction due to these currents prevented any sort of an image of the crater being formed while the pressure was varying. While the pressure was steady a good image could be formed. This tube is nearly 3 ft. in length, and only $\frac{1}{2}$ in. in bore, and it would naturally take time for the gas to settle down throughout its length. We propose to have this tube removed, and the aperture in the box closed by a strong piece of plain glass, and to form an image of the carbons by a lens placed at a suitable distance outside. This we expect will remove the difficulty arising from these convection currents.

The result of all these experiments so far is that it would require more evidence than we have been able to get, to affirm that either the temperature of the crater of the arc is raised or lowered by pressure. We got some very concordant observations, which showed the temperature to be lowered with pressure, and in which at the time we could see no evidence of absorption by fog, but then, at other

times, there was undoubtedly absorption from this cause. We certainly got no evidence that there is any appreciable increase of temperature. When the arc was started in the gas at a low pressure and then the pressure was raised, the radiation at the low pressure was greater than at a high pressure; but when the arc was started first in the gas at high pressure, and then the pressure reduced, the radiation was rather higher in the gas at high pressure. From all this we concluded that the greater part of the differences we were observing were due to the absorption of the light in the long tube already mentioned, which increased the longer the arc was kept burning, and was probably greater at high than at low pressures. The best observations were made with variations of pressure from 15 up to 100 lbs. per sq. in., and there seems very little evidence of much change of radiation with this change of from 1 up to between 6 and 7 atmos.

The whole question is surrounded with great difficulty. If the carbon be really in equilibrium with its own vapour at the temperature of the crater and at the pressure of the surrounding atmosphere, some relation must exist between the change in pressure and change in temperature of the crater. If we knew the latent heat of volatilisation of carbon, we should be able to calculate the change of temperature from the well-known thermodynamic formula

$$\frac{\delta T}{T} = \frac{\Delta v}{\lambda} \cdot \delta p.$$

Δv can certainly be approximately determined on the supposition that the absolute temperature of the crater is fifteen times the absolute temperature of the freezing point, *i.e.*, 3800. We thus get for gaseous carbon $\Delta v = 10^4$, *q.p.*, at this temperature. For 1 atmos. $\delta p = 10^6$, *q.p.*, so that

$$\frac{\delta T}{T} = \frac{10^{10}}{\lambda}.$$

Hence, unless the latent heat of carbon be enormously great compared with that of other substances, $\delta T/T$ will be considerable. If λ be as great as the latent heat of vaporisation of carbon given by Trouton's law, *i.e.*, about 4000 calories, or 16.8×10^{10} ergs, $\delta T/T$ would be about $\frac{1}{17}$, and δT would be nearly 220° C. for each atmosphere, and a change of pressure of about 18 atmos. would raise the temperature of the crater to that estimated for the sun. The corresponding increase of radiation would be very great, for the radiation varies, at least approximately, as the fourth power of the absolute temperature. This would lead one to expect that the radiation would be nearly doubled for each 4 atmos. added. Such an increase as this certainly does not take place, so that we may conclude that either the temperature of the crater is not that of boiling carbon,

or else that the latent heat of volatilisation of carbon is very considerably greater than that calculated from Trouton's law. Even though this latent heat were as great as the heat of combustion of C to CO_2 , *i.e.*, 7770, there would be an increase of about 70 per cent. in the radiation for an increased pressure of 6 atmos. Such an enormous latent heat is unprecedented, and yet our experiments would, almost certainly, have shown such an increased radiation as this. So far, therefore, the experiments throw considerable doubt on the probability that it is the boiling point of carbon that determines the temperature of the crater. It might be questioned whether there is energy enough in the current to do all this work, but upon an extravagant estimate of the amount of carbon volatilised in the crater, it appears that there is more than a hundred times as much energy supplied by the current as would be required for volatilising the carbon, even though its latent heat were as great as the heat of combustion of C into CO_2 .

There is another considerable difficulty in the theory of the temperature of the crater being that of boiling carbon arising from the slowness of evaporation. The crater on mercury is dark, but then it volatilises with immense rapidity and the supply of energy by the current being more than 100 times that required merely for evaporation, there seems very little reason why even a considerable difference in latent heat should make any sensible difference in the rate of evaporation of mercury and carbon, especially as, at the same temperature, the diffusion of carbon vapour is nearly three times as fast as that of mercury vapour and the temperature immensely higher.

We would, in conclusion, call attention to a cause of opacity in the solar atmosphere that is illustrated by the effect of convection currents in the long tube we were observing at high pressures; these convection currents behaved just like snow, or any other finely divided transparent body immersed in another of different refractive index. Light trying to get through is reflected backwards and forwards in every direction, until most of it gets back by the way it came. The consequence was that even the electric arc light was unable to penetrate the tube at high pressure, when these convection currents were active. The only light that came out of the tube was the feeble light outside, which was returned to us by reflection at the surfaces of these convection currents. In a similar manner we conceive that any part of the solar atmosphere which is at a high pressure, and where convection currents, or currents of different kinds of materials, are active, would reflect back to the sun any radiations coming from below, and reflect to us only the feeble radiations coming from interplanetary space. In his paper on "The Physical Constitution of the Sun and Stars" ('Roy. Soc. Proc.,' No. 105, 1868), Dr. Stoney called attention to an action of this kind that might be due to clouds of transparent

material, like clouds of water on the earth, but in view of the high solar temperature it seems improbable that any body, except, perhaps, carbon, could exist in any condition other than the gaseous state in the solar atmosphere; so that it seems more probable that sun-spots are due, at least partly, to reflection by convection streams of gas, rather than by clouds of transparent solid or liquid particles.

“Influence of Alterations of Temperature upon the Electrotonic Currents of Medullated Nerve.”* By AUGUSTUS D. WALLER, M.D., F.R.S. Received December 14,—Read December 17, 1896.

(Abstract.)

The effects of a rise of temperature upon electrotonic currents may be briefly stated as follows:—

1. The ordinary electrotonic currents, A and K, are temporarily diminished or abolished at about 40° .

2. At about 30° of a rising temperature the K current is increased without notable alteration or with actual diminution of the A current.

3. On returning from 40° towards the normal ($15^{\circ} \pm 2^{\circ}$) temperature, the A and K currents reappear. K is increased and A is diminished, so that the previous normal inequality $A > K$ is diminished, or actually reversed to $A < K$. In all cases the quotient A/K is diminished; in some cases it actually falls below unity.

[The negative variation is temporarily abolished at about 40° ; a positive gives place to a negative variation in consequence of a raised temperature to 40° .]

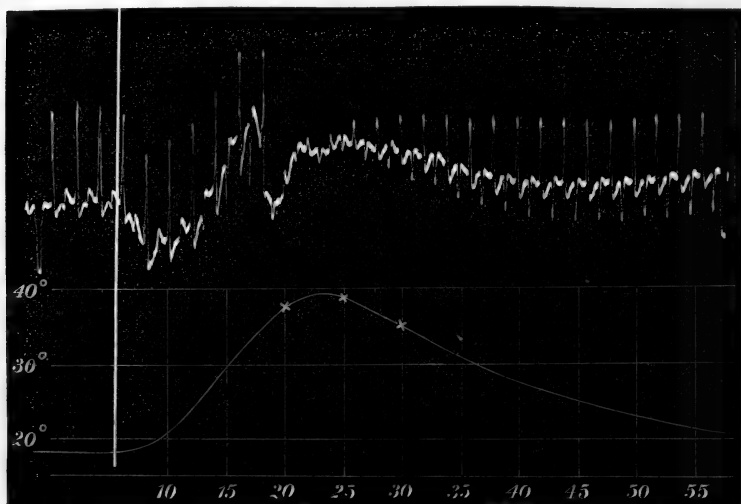
The above three statements are illustrated by Experiments 2366, 2322, and, from the examination of their records, it will be clear that there is here no question of the effects being due to alterations of resistance. A and K are tested for alternately, and the deflection by 0.001 volt is taken at intervals of about ten minutes. [Other examples of a similar character are given in the ‘Proceedings of the Physiological Society’ for November, 1896, and a record of temporary diminution of the negative variation is given in fig. 12 (Experiment 777), ‘Phil. Trans.’ 1897.]

* In all the experiments referred to in this communication, the polarising current is by one Leclanché cell (the resistance in its circuit being about 100,000 ohms). The nerve lies upon four unpolarisable electrodes fixed at intervals of 12 mm., serving as leading-in electrodes to the polarising current and leading-out electrodes to the electrotonic current. On the galvanometer records, the anelectrotonic deflection A reads upwards, the katelectrotonic deflection K reads downwards; after-anelectrotonic and after-katelectrotonic deflections A' and K' read respectively downwards and upwards (there being under the conditions of experiment no marked homodromous after-katelectrotonic deflection).

Exp. 2322.—Influence of raised Temperature upon Anelectrotonic and Katelectrotonic Currents.

Time.	Temperature.	A.	A'.	K.	K'.	$\frac{1}{1000}$ volt.
0 min.	17°	—	—	—	—	9
1	"	+12	-2	—	—	
2	"	—	—	-trace	+2	
5	"	+12	-2	—	—	
6	"	—	—	-trace	+2	
Heat.	10	+12.5	-2.5	—	—	
	11	—	—	-trace	+3	
	15	—	—	-5	+3.5	
	16	+11.5	-5	—	—	
	20	+3	—	—	—	
	21	—	—	-1	—	
	25	—	—	-2	—	
	26	+3	—	—	—	
	30	+5	-1.5	—	—	
31	35	—	—	-3.5	+0.5	9
40	28	+8.5	-2.5	—	—	
41	—	—	—	-4.5	+1	
50	24	+8.5	-2	—	—	
51	—	—	—	-4.5	+1	
52	—	—	—	—	—	

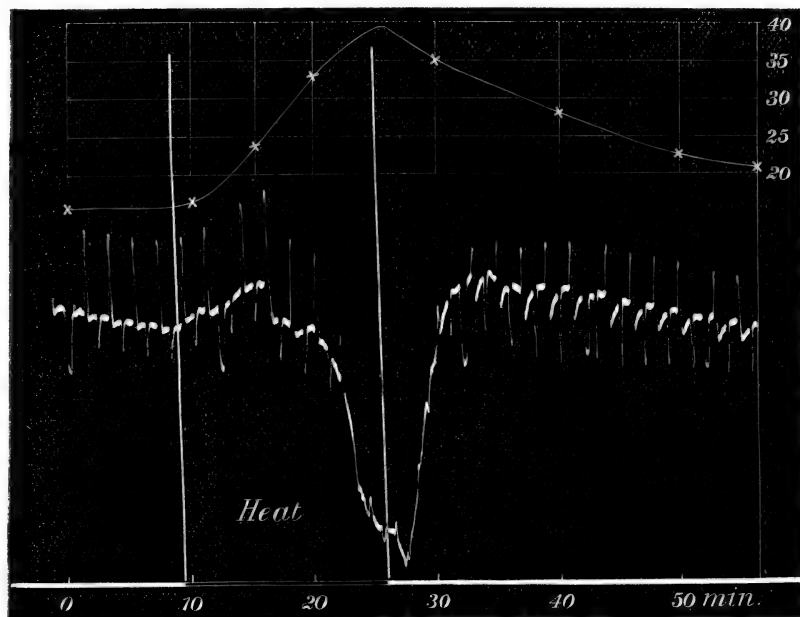
The K current is very small, the K' after-current is comparatively large. In consequence of heating to 39.5°, K is increased, A and K' are diminished. The quotient A/K is diminished.



Exp. 2366.—Influence of raised Temperature upon Anelectrotonic and Katelectrotonic Currents.

Time.	Temperature.	A.	K.	$\frac{1}{1000}$ volt.
0 min.	16°	—	—	8
1	"	+11·5	—	
2	"	—	-4·5	
7	"	+11·5	—	
8	"	—	-4	
Heat. {	12	—	—	8
	14	+12	—	
	15	—	-4·5	
	16	+12	—	
	17	—	-5	
	18	+11	—	
	19	—	—	
	20	+10	—	
	21	—	-5	
	25	+2	—	
26	40	—	1·5	
30	36	+4	—	
31	35	—	-4	
32	34	—	—	10
33	33	+4	—	
34	32	—	-7	
42	26·5	—	—	7
43	26	—	-8·5	
44	25·5	+6·5	—	
52	22·5	—	—	6·5
53	22	+6	—	
54	22	—	-6	

After heating to 40° the A current is diminished, the K current is increased, and a well-marked A' after-current has developed. The quotient A/K is diminished.



Electrotonic after-currents, A' and K', when present to any marked degree, are opposed to the previous electrotonic currents A and K. Designating A and K respectively as positive and negative, the after-currents A' and K' are respectively negative and positive. Such after-currents are in general modified by previous rise of temperature, which gives rise to an evident A' (negative) in a nerve which previously gave no marked A', and abolishes a K' (positive) that may previously have been present. Experiment 2366 exhibits the development of an evident negative A' subsequent to heating of the nerve. Experiment 2322 exhibits the abolition of a positive K', evident previous to heating of the nerve.

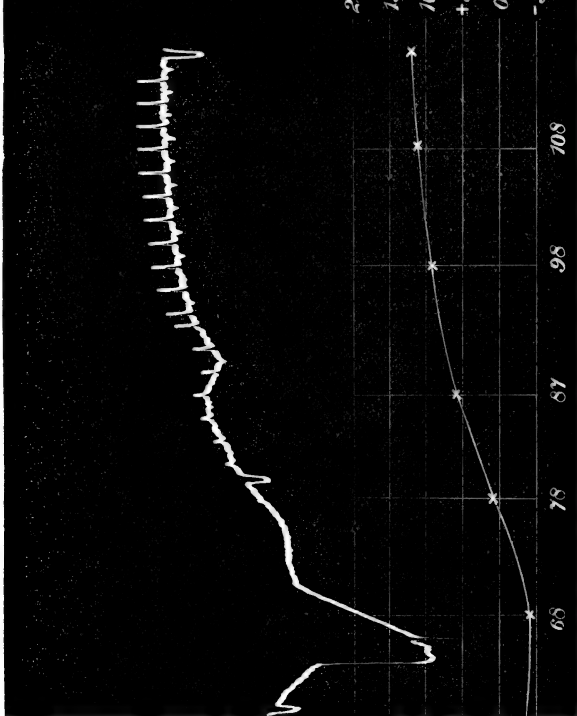
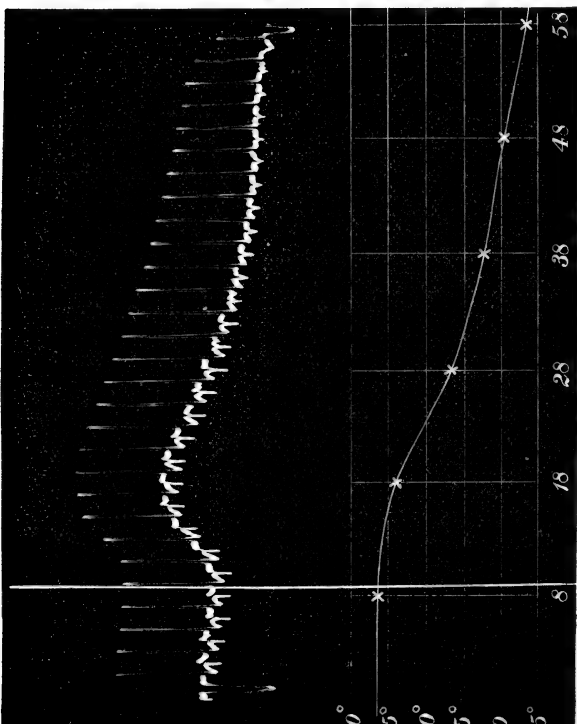
A fall of temperature causes an increase of the A current and, in less degree, of the K current; by reason of the diminution of resistance that takes place with lowered temperature, the increase of A is more marked than is apparent upon the record, and the smaller increase of K is quite masked by the diminution of resistance. The quotient A/K is augmented. At a temperature of -4° to -6° both currents are somewhat suddenly abolished; this abolition may be complete and final, no recovery taking place, or it may be temporary, being succeeded by imperfect recovery as the nerve temperature returns towards normal. It is noteworthy that the A and K currents are not abolished at 0° suddenly, and all but finally abolished at -4°

to -6° , probably by reason of the nerve having been frozen at this temperature and thus cut to pieces.

It is evident that little stress is to be laid upon an apparent decrease of K with falling temperature (2417) and increase of K with rising temperature (2366). On the other hand, a diminished A with rising temperature (2366) and an increased A with falling temperature (2417) are not open to doubt.

Exp. 2334-5.—Influence of lowered Temperature upon Anelectrotonic and Katelectrotonic Currents.

Time.	Temp.	A.	K.	$\frac{1}{1000}$ volt.
0 min.	17°	—	—	9.0
1	"	+11.5	—	
2	"	—	-2.5	
7	"	+11.5	—	
8	"	—	-2.5	
Cold.	9	+11.5	—	
	10	—	-2.5	
	17	+11.5	—	
	18	—	-2.5	
	27	+12	—	
	28	—	-2.5	
	37	+12	—	
	38	—	-1.5	
	47	+10.5	—	
	48	—	-1	
	56	—	-0.5	
	57	+6.5	—	
	58	—	—	3.5
	67	0	—	
68	-4	—	0	
77	+1.5	0	—	
78	+2	—	0	
80	4	—	—	3.5
87	6	+2	—	
88	6	—	-trace	
98	9	+3.5	—	
99	9.5	—	-0.75	
108	11	+3.5	—	
109	11.5	—	-1	
116	12	—	—	4.5
180	14	+4	-2	6.0



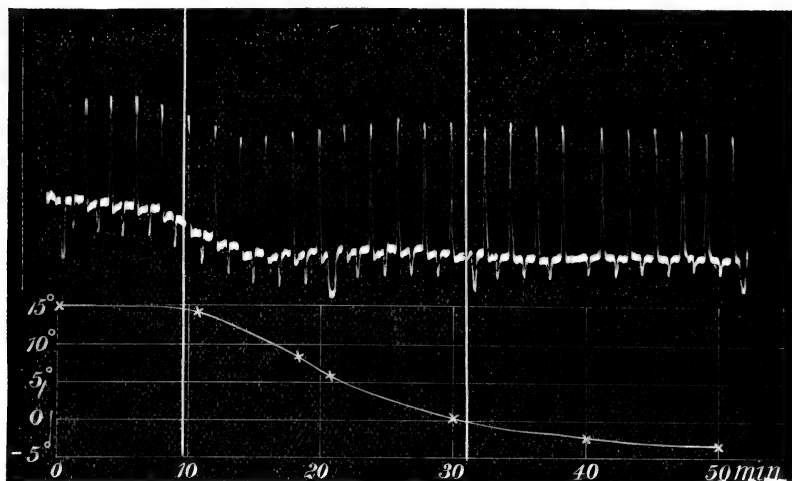
Expt. 2417.—Effect of Cold on A and K.

Time.	Temperature.	A.	K.	A/K.	$\frac{1}{1000}$ volt.
0	15°	—	—	—	7·5
1	—	—	-4	—	—
2	—	+13	—	3·25	—
7	—	—	-4	—	—
8	—	+13·5	—	3·37	—
11	14	—	-4	—	—
12	—	+14·5	—	3·62	—
13	—	—	-4	—	—
14	12·5	+14·5	—	3·62	—
15	—	—	-3·5	—	—
16	11	+15	—	4·28	—
17	—	—	-4	—	—
18	9	+15·5	—	3·87	—
21	6	—	—	—	5·5
23	4	—	-3·5	—	—
24	3	+16·5	—	4·71	—
27	2	—	-3·5	—	—
28	1·5	+16·5	—	4·71	—
29	—	—	-3	—	—
30	0·5	+17	—	5·66	—
32	—	—	—	—	4·5
33	-0·5	+16·5	—	—	—
34	—	—	-3	—	—
35	1	+17	—	5·66	—
36	—	—	-2·5	—	—
37	-1·5	+17	—	6·8	—
38	—	—	-2·5	—	—
39	-2	+17·5	—	7	—
40	—	—	-2·5	—	—
41	-2·5	+17	—	6·8	—
42	—	—	-2·5	—	—
43	-3	+17	—	6·8	—
44	—	—	-2	—	—
45	-3	+16·5	—	8·25	—
48	—	—	-2	—	—
49	-3·5	+16·5	—	8·25	—
52	—	—	—	—	4

The A effect obviously increases with fall of temperature (increasing resistance); the K effect apparently diminishes, but actually increases a little, the increase being masked by increased resistance. The A/K quotient is obviously increased.

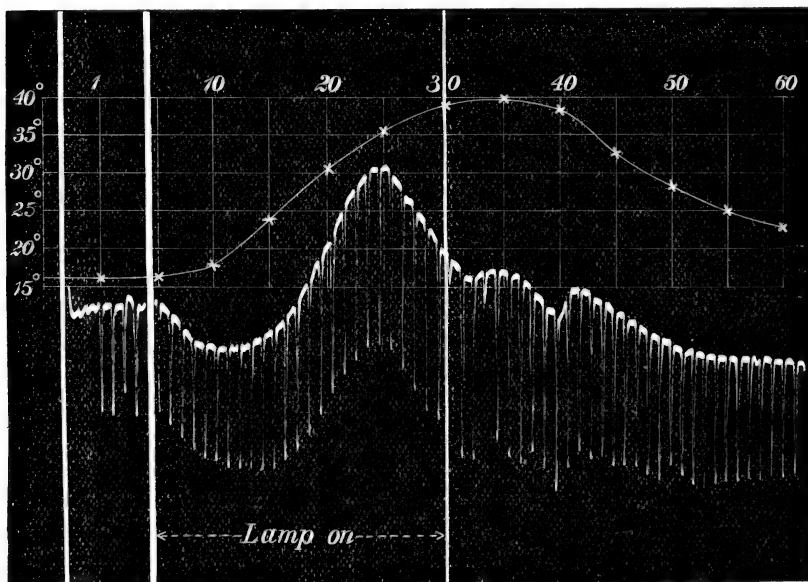
The voltage calculated from the data of this experiment is:—

At 15°	A = 0·00173 volt,	K = 0·00053 volt.
„ 10	A = 0·00244 „	K = 0·00059 „
„ 5	A = 0·00285 „	K = 0·00064 „
„ 0	A = 0·00360 „	K = 0·00070 „



Exp. 2344.—Influence of Alterations of Temperature upon the Electrical Resistance of Nerve.

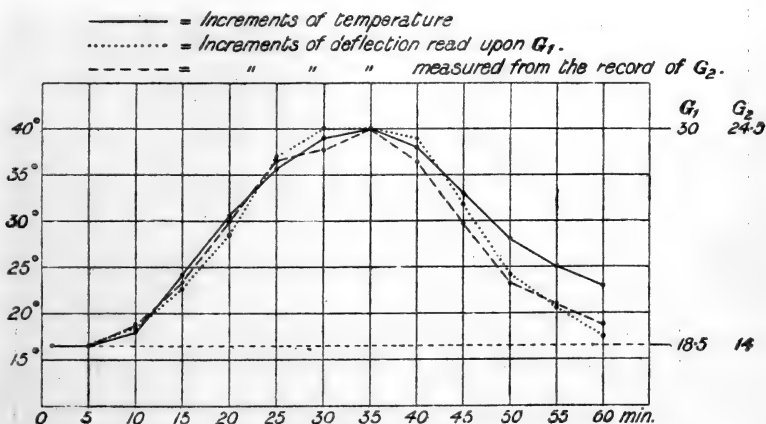
The following experiment (2344) made to test the effect of rising and falling temperature upon the electrical resistance of nerve, and the value attaching to observations of a standard deflection by constant E.M.F. as an indication of altered resistance, shows very



clearly that such standard deflection gives measure not only of the electrical resistance, but also—due reservation being made of the effect of drying in the course of a prolonged observation at raised temperature—is itself available in measure of the alteration of temperature of the nerve.

Exp. 2344.—Deflections by a small constant E.M.F. (0.002 volt) through a Nerve at rising and falling Temperature and through two Galvanometers.

Time.	Thermometer.	Demonstrating galvanometer, G_1 .	Recording galvanometer, G_2 .
1 min.	16.5°	18.5 c.m.	14.0 mm.
5	16.5	18.5 "	14.0 "
10	18.0	19.5 "	15.0 "
15	24.0	21.5 "	17.0 "
20	30.5	25.5 "	20.0 "
25	35.5	28.5 "	23.0 "
30	39.0	30.0 "	23.5 "
35	40.0	30.0 "	24.5 "
40	38.0	29.0 "	23.0 "
45	33.0	26.0 "	20.0 "
50	28.0	22.0 "	17.0 "
55	25.0	20.5 "	16.0 "
60	23.0	19.0 "	15.0 "



[Experiments on the comparative effects of acids and bases upon the A and K currents, have shown that within a certain moderate range of concentration (soakage of the nerve in $n/15$ to $n/20$ solution for one minute) acid favours the K current and disfavours the A

	Plate No.	Before.			Reagent.	After.			Remarks.
		A.	K.	A/K.		A.	K.	A/K.	
1	2322	+12	- 0.5	24	Heat to 39°·5	+ 5	- 3.5	1.4	30—31st min. of experiment.
2	2366	+11.5	- 4	2.9	Heat to 40°	+ 4	- 7	0.57	33—34th min. of experiment.
3	2334-5	+11.5	- 2.5	4.6	Cold to -0°·5	+10.5	- 1	10.5	47—48th min. of experiment.
4	2417-8	+13	- 4	3.2	Cold to +1°	+16.5	- 3	5.7	28—29th min. and 44—45th min. of experiment.
5	2363	+17	-12	1.4	And to -3°·5	+ 2.5	- 3.5	0.7	Immediately and 10 mins. later.
6	2158	+19.5	- 3	6.5	Carbonic acid for 2 mins.	+19	-15.5	1.2	Immediately and 10 mins. later.
7	2412	+14	- 3.5	4.0	Carbonic acid for 3 mins.	+ 8	- 3.5	2.3	Immediately and 10 mins. later.
8	2354	+30	-12.5	2.4	Propionic acid, $n/10$, for 1 min.	+24	- 5	4.8	5—6th min. after.
9	2355	+22.5	- 7	3.2	Sodium hydrate, $n/20$, for 1 min.	+ 6	- 4.5	1.3	13th min. and 30 mins. later.
10	2360	+19	- 8	2.4	Sodium hydrate, $n/20$, for 1 min.	+14.5	- 6.5	2.3	2 or 3 mins. after.
11	2372	+21	- 8	2.6	Potassium hydrate, $n/20$, for 1 min.	+23	- 3.5	6.6	4—5th min. and 30 mins. later.
12	2373	+28	- 9	3.1	Ether (Et_2O) for 8 mins.	+14.5	- 2	7.2	Immediately and 10 and 30 mins. later.
					Chloroform for 8 mins.	+18	- 2.3	7.8	Immediately and 10 and 30 mins. later.
						+15	- 1.5	10.0	
						+ 8.5	- 7.5	1.1	
						+22.5	- 7.5	3.0	
						+20	- 8	2.5	
						+11	-11	1.0	
						+ 2.5	- 1.5	1.7	
						+ 1.5	- 1.5	1.0	

current, giving decrease of the quotient A/K ; while base favours the A current and disfavours the K current. In other words, the anodic or acidic polarisation is favoured by base, disfavoured by acid; the cathodic or basic polarisation is favoured by acid, disfavoured by base. Anæsthetics (CO_2 ; Et_2O ; CHCl_3) act like acids and like rise of temperature, causing, at certain strengths, a greater relative diminution of A than of K, and therefore a diminution of the quotient A/K —temporary in the case of CO_2 and Et_2O , permanent in the case of CHCl_3 . In the weakest dilution that will produce any effect at all there may be increase of A, no increase, or a relatively smaller increase, of K, and therefore increase of the quotient A/K . These effects are, however, at present under examination, and will form the subject of a future communication. The tabular summary (p. 391) will at this juncture be sufficient to enable a comparison to be made between the effects of heat and cold and those of acids and alkalies.]

“On the Occurrence of Gallium in the Clay-ironstone of the Cleveland District of Yorkshire: Determination of Gallium in Blast-furnace Iron from Middlesbrough.” By W. N. HARTLEY, F.R.S., Professor of Chemistry, and HUGH RAMAGE, A.R.C.Sc.I., F.I.C., Assistant Chemist, Royal College of Science, Dublin. Received December 2,—Read December 17, 1896.

In the month of April of this year, we had the honour to submit to the Royal Society* a preliminary notice of the evidence we had obtained of the existence of gallium in the Yorkshire ironstone smelted at Middlesbrough-on-Tees.

We propose now to give a concise but detailed account of the methods of analysis carried out on the metal and the ore, and the determination of the quantity of gallium present.

Examination of the Blast Furnace Metal.

Method of Analysis.—The very large proportion of iron rendered the application of some special method of analysis necessary for the separation of metals present in minute proportions, and for the qualitative and quantitative examinations of the separated substances. We have successfully employed fractional precipitations and the spectrographic analysis of the precipitates, supplemented by gravimetric determinations of the purified gallium sesquioxide.

The sample of metal first received consisted of two small ingots, each weighing about 230 grams; small pieces, broken with difficulty,

* ‘Roy. Soc. Proc.’ vol. 60, p. 35, 1896.

from these and heated in the oxyhydrogen flame, gave the gallium line $\lambda 4171.6$. The ingots were exceedingly hard, and practically resisted all attempts to reduce them to small pieces. One ingot, weighing 210 grams, was boiled with hydrochloric acid, until its solvent action on the metal had nearly ceased, when the liquid was decanted, and fresh acid poured on. The process was very slow, and, after 80 grams had been dissolved, the remaining piece of metal was scraped, to remove an adhering layer of carbonaceous matter, and the analysis of the products proceeded with. The liquid was filtered, and the black residue *A* washed. The filtrate was evaporated, to expel the excess of hydrochloric acid, water was added, and the solution, not being clear, was filtered. The residue *B* thus obtained had, when dry, a dark greyish colour. *Residues A and B.*

In the clear filtrate two rods of zinc were immersed, and during a period of one hour and three-quarters hydrogen was evolved, and metals were deposited on the zinc. The deposit was scraped off, and separated from the liquid by filtration. *Metallic deposit C.* To the filtered solution were added about 4 c.c. of lead acetate solution, and two rods of zinc were placed in the liquid, according to the method of Lecocq de Boisbaudran,* by which, as the lead is precipitated, traces of other metals, such as copper, silver, indium, thallium, &c., are collected by the lead. *Metallic precipitate F.*

Fractional Precipitation by Ammonium Acetate.—The filtrate, in volume about 2 litres, was boiled, but as no precipitate formed, 15 c.c. or thereabouts of a solution of ammonium acetate were slowly added, and the solution boiled; the iron, being in the ferrous state, was retained in solution, whilst it was expected that the gallium would be precipitated as phosphate. After boiling for about twenty minutes, the substances precipitated were collected on a filter and washed. *Residue D.*

The filtrate was again boiled with about 10 c.c. of ammonium acetate, and the precipitate collected on a filter. *Residue E.*

Further Precipitation of Basic Acetates.—The filtrate from *E* was again boiled with ammonium acetate, the resulting precipitate being filtered off. It was much darker in colour than those previously obtained. *Sesquioxide metals G.*

The filtrate was evaporated until it became a saturated solution of ferrous chloride. It was allowed to cool and crystallise, and the operation was repeated upon the mother liquor. The two crops of crystals were mixed with others, which were obtained as follows. The solution from the remaining portion of the 210 grams of metal was filtered, and the filtrate evaporated. It was then allowed to cool and crystallise. The mother liquor was concentrated, and again allowed to cool and crystallise, the different crops of crystals

* 'Spectres Lumineux.'

being collected. The mother liquor from the last crop of crystals was evaporated almost to dryness to expel acid, and, after addition of water, rods of zinc were immersed in the solution, which was then left undisturbed for forty-eight hours. The zinc was found to have been almost all dissolved. The precipitated metals and the residue of zinc were washed and dried. *Residue H.* The filtrate, after three precipitations with ammonium acetate, was mixed with the mother liquor of the ferrous chloride crystals from the first portion, so that the liquid then represented the whole of the ingot. It was diluted, mixed with an excess of ammonia and ammonium sulphide, to precipitate all the iron and metals of that group still in the solution, and filtered. The filtrate was evaporated to dryness and gently ignited to expel ammonium salts. A residue was left, which contained the alkaline earths and alkaline metals.

The Spectrographic Analysis of the Residues and of the Precipitates.

From the foregoing description it will be observed that by partial solution the metals precipitable by iron may be looked for along with carbon, and, probably, some phosphides of iron and other metals. Such phosphides yield the flame spectra of the metals only, and not of the phosphorus combined with them. Precipitation with zinc in an acid solution was expected to give a deposit (*F* and *H*) which would yield the spectra of copper, silver, bismuth, lead, thallium, and tin in the oxyhydrogen flame, if these metals were not already precipitated by the iron, and present in the residues *A* and *B*; while *D*, *E*, and *G* are compounds which fall under the category of sesquioxide metals, including beryllium, aluminium, indium, gallium, and chromium. Of these, aluminium and beryllium were expected to show no spectra in the oxyhydrogen flame, and for these it was intended to use spark spectra.

The residue *A*, when dried in the water oven and gently heated, gave off fumes which indicated that an oil was present, and extraction with ether and subsequent evaporation did, indeed, yield a quantity of a brown oil.

The oxyhydrogen flame spectra of the substances separated were photographed, and the following are particulars regarding their spectra.

The insoluble residue *A* contained iron, manganese, copper, gallium, sodium, chromium, silver, and nickel.

The lines which served to identify the metals had the following wave-lengths:—

Iron	4208.0	4046.0	3929.8	3922.0	3904.8
	3898.5	3886.5	3860.0	3857.0	3841.0
	3834.0	3826.0	3824.5	3758.4	

and all the strong lines in the groups extending to 3441, corresponding to the solar line O.

Manganese .	4033·0	4032·0	4030·0	—	—	—
Copper.....	3290·0	3262·5	—	—	—	—
Gallium....	4171·6	4032·7	—	—	—	—
Sodium	5893·0	5688·0	4668·0	3303·0	—	—
Chromium..	4289·0	4274·0	4253·0	3606·0	3593·0	3578·0
Silver.....	3383·5	3282·1	—	—	—	—
Nickel	3525·0	3415·0	—	—	—	—

The insoluble residue B contained iron, copper, sodium, and a trace of potassium.

Iron.....	Groups of lines lying between 4045·0 and 3440·0					
Copper...	Lines with wave-lengths....	3290·0	„	3262·5		
Sodium..	„	„	5893·0	„	3303·0
Potassium	„	„	4047·1	„	4043·5

The metals precipitated by zinc, C. These were iron, copper, silver, a trace of lead, also some sodium and potassium. There was also a trace of chromium, and this, like the trace of iron, was probably precipitated as basic chloride or as hydroxide. The wave-lengths of the lines of iron, silver, and copper need not be recapitulated.

Lead	4057	3682 and 3639
Chromium.....	4289	4274 „ 4253

The metals precipitated by zinc after addition of lead acetate, F.—The metallic deposit yielded a complex spectrum containing the lines already mentioned of the following elements: iron, chromium, copper, silver, gallium (*a trace*), potassium, sodium, and, of course, lead, as this had been added.

The lead here appears as a banded spectrum, the edges of the bands seen being those at wave-lengths:—

5675	5460	4980	4824	4657
4597	4370	4314	4225	4140
4061	4057	3985 and 3954		
Nickel lines....	3525	„	3415	

The copper lines were very strong, the silver weak.
The potassium lines were the following:—

4047·1 4043·5 strong,
then much fainter—
3447·5 and 3446·5.

The precipitates of Phosphates and basic acetates D, E, and G.

Precipitate E.

Chromium	5206	4289.0	4274.0	4253.0
	3606	3594.0	3579.0	
Gallium.....	strong	4171.6 and	4032.7	
		(the latter somewhat weaker).		
Calcium.....	weak	4226.8		
Potassium.....	strong	4047.0	4043.5	
Sodium.....	strong	5893.0, faint	5685, and	3303.0

Precipitate D.

The chromium line 5206 did not appear in the spectrum of this precipitate. Both the gallium lines were very distinct, 4171.6 and 4032.7.

It is remarkable how very generally the spectrum of potassium appears along with that of the precipitated substances, whether metals or basic acetates.

Precipitate of basic acetates, G.

This contained iron, chromium, lead, gallium, potassium, and sodium. The lines were those which have already been particularised.

The Residue left by Zinc, H.—This was heated with aqua regia, when all but a very small quantity of silica with a trace of a metallic oxide dissolved. The liquid was filtered and the filtrate evaporated with excess of hydrochloric acid to remove nitric acid. It was diluted with water, when it showed a green colour.

It was saturated with sulphuretted hydrogen and filtered to separate the precipitate. The precipitate was partially soluble in sodium hydrogen sulphide, yielding a sherry-coloured solution; the constituent causing this colour was not identified, the quantity present being very small. The residue, insoluble in alkaline sulphide, contained copper and a trace of lead, but no mercury, bismuth, or cadmium.

The filtrate from zinc and precipitated metals I, was diluted and heated to boiling. It gave a precipitate, and therefore ammonium acetate was added to the hot liquid, and after boiling for several minutes it was filtered. The filtrate became turbid immediately; it was then boiled and more ammonium acetate added and then filtered; the filtrate again became turbid.

This precipitate was filtered off and heated in the oxyhydrogen flame.

It contained no gallium, but the spectrum gave lines of iron, copper, sodium, potassium, and a trace of lead.

It is evident that all the gallium was extracted by the repeated additions of ammonium acetate solution and boiling.

The various precipitates of basic acetates were mixed, with the exception of that from *I*, which contained no gallium. In order to separate phosphoric acid, the precipitates were fused with about three times their weight of mixed carbonates. Some potassium nitrate was added towards the end of the fusion, to convert chromium into chromates. The heavy metals were left as oxides or carbonates, the phosphoric acid going into solution. After extraction with hot water, the solution was filtered.

Filtrate L.

Coloured greenish by manganates, boiled with a few drops of alcohol to separate manganese as hydroxide. Solution, after again filtering from manganese, was yellow from chromates.

Residue M.

Dried and fused in a silver dish with caustic soda to dissolve gallium hydroxide. Extracted with water and filtered. *Residue* not examined further. *Solution*: acidified with HCl and ammonium chloride and ammonia added. The precipitate was filtered off, dissolved in HCl, and sparked to observe its spectrum.

These gallium spectra showed that there were still traces of chromium in the gallium chloride, and from this the gallium was purified completely by precipitation in a strongly acid solution with potassium ferrocyanide and subsequent removal of the iron by treatment with sodium hydrate, according to the method of Lecocq de Boisbaudran.*

The foregoing description of the analytical details proves the presence of gallium in the metal, and gives a clear indication of how it may be separated by a simple process.

In subsequent operations on the blast-furnace metal, the ferrous chloride was mixed with calcium carbonate, and the gallium was found to be all precipitated and capable of easy separation from the calcium salt.† Latterly it was found to be more convenient to boil the acid solution containing gallium with an excess of the iron under examination, and thus the gallium is concentrated in the residue which remains undissolved.‡

It became necessary to consider what was the source of the gallium contained in the iron. Was the gallium concentrated in the metal? Or did it pass into the slag of the converter? Was it originally contained in the ore, the lime, or the fuel? Was it easily volatilised, so as to pass off with fume or with flue dust?

* 'Comptes Rendus,' vol. 94, p. 1228.

† *Loc. cit.*, p. 1629.

‡ 'Comptes Rendus,' vol. 49, p. 1625.

On February 10th we received from Mr. C. R. Ridsdale, the Chemist at the North Eastern Co.'s Steel Works, at Middlesbrough, samples of the following materials :—

1. " Mixer metal," *i.e.*, mixed blast-furnace metal.
2. Roasted Cleveland iron ore.
3. Flue dust.
4. Tap cinder.
5. Manganese ore.
6. Lime.

On February 12th, photographs of the oxyhydrogen flame spectra of these substances were obtained.

The following are the particulars of this examination :—

1. The roasted Cleveland ore contained iron, sodium, potassium, manganese, chromium, nickel, copper, gallium, lead, and calcium.
2. The blast-furnace metal contained iron, sodium, potassium, manganese, nickel, copper, gallium, and lead.
3. Flue dust contained iron, sodium, potassium, manganese, chromium, nickel, copper, silver, gallium (doubtful), lead (strong), calcium, and rubidium. Rubidium was identified by the lines 4202 and 4216. (Thalèn.) Calcium by line 4226, in the blue.

It is evident now that gallium is contained in the ore and is concentrated in the metal.

1. The manganese ore (a 15 per cent. Spanish ore) contained iron, sodium, potassium, manganese, copper, silver, lead, indium, and calcium. The lines by which the indium and the silver were identified are as follows :—

Indium	4510·2	4101·3
Silver	3383·5	3282·1

The occurrence of indium is remarkable, as hitherto it has been found only in zinc blendes.

2. Tap cinder contained iron, sodium, potassium, manganese, copper, and lead.
3. Lime contained calcium, magnesium, potassium, and sodium, a trace of iron, and a trace of manganese.

The lime showed the following bands, characteristic of lime* :—

Band in the orange from 6253 to 6116, degraded towards the more refrangible side.

Band from 6075 to about 5900.

* ' Phil. Trans., ' vol. 185, p. 182.

Very strong band from 5598 to 5485.

Band of continuous rays with other bands discernible in it.

Less refrangible edge of band 5445.

Band in the same at 5422, 5390, 5359, 5341, 5322.

The more refrangible edge of band 5304.

Very narrow band in the blue, more like a very strong broad line from 4222 to 4215.

The magnesium oxide was identified by three bands, more or less connected by diffused rays.

1st. From 3929 to 3856

2nd. „ 3834 „ 3805

3rd. „ 3805 „ 3682

On these bands were seen ten iron lines, six in the first principal group and four in the second, all very faint, but with apparently the following wave-lengths, which correspond with the lines seen in oxyhydrogen flame spectrum of ferric oxide. They are also closely in approximation to, and probably identical with, the following arc lines, measured by Kayser and Runge in iron.

3860.03	3856.49	3826.04	3824.58
3758.36	3748.39	3745.67	3737.27
3735.0	3722.69	3720.07	

Roasted Cleveland Iron Ore. Process for the Extraction of Gallium.

This ore is a complex substance, and contains elements which render the complete extraction of the gallium very difficult. It is in great part soluble in strong hydrochloric acid, but the iron goes into solutions as a ferric salt, and difficulties arise in attempting to reduce it to the ferrous state. Zinc and iron are both liable to contain gallium, and, without a very careful examination of a quantity of the metal, it would be wrong to use them as reducing agents, seeing that the quantity of metal required in the process is large in comparison with the sample treated. Sulphurous acid and kindred substances yield sulphates which cause a quantity of the alkaline earths to separate as sulphates, and, as these precipitate in faintly acid solutions, there is a risk of basic gallium sulphate being carried down with them.

Dilute hydrochloric acid yields a solution poor in iron, but the dissolved matter is richer in gallium than the original ore. A large proportion of silicic acid is, however, contained in the solution.

Experiments were made on quantities of 50 grams of the ore, and the spectra from the sesquioxide metals were carefully compared with the spectra from the similar products from the metal, and we find that, as in the comparison of the original samples of ore and

metal, the gallium lines are decidedly stronger in the spectra of the substances extracted from the metal.

One kilo. of finely powdered ore was mixed with dilute hydrochloric acid of double normal strength, measuring about 1250 c.c. Some carbon dioxide was disengaged and an insoluble residue left which was removed by filtration. The filtrate was then heated when a gelatinous separation of silica occurred. After evaporation to dryness, a further addition of hydrochloric acid yielded a solution which was not highly coloured, and, presumably, did not contain much iron. The silica rendered insoluble was removed by filtration, and to the filtrate ammonium chloride and ammonia were added. The precipitate thus formed was dissolved in hydrochloric acid, reduced with sulphur dioxide, nearly neutralised, and boiled with sodium thiosulphate. The precipitate was dissolved in hydrochloric acid and again precipitated by ammonia.

This precipitate was examined for gallium. The insoluble residue was also examined, and a comparison of the two spectra showed that a larger quantity of gallium remained in the insoluble residue than was extracted by the acid. It was found that gallium could be extracted from this by fusion with caustic soda and lixiviation with water, and that the residue, after such treatment, contained no gallium. Operations on this particular ore were suspended until other samples had been examined.

The following ores from the collection in the Royal College of Science, Dublin, were examined:—

1. Yorkshire clay ironstone from near Middlesbrough.
2. Clay ironstone from Grosmont, Whitby, Yorkshire.
3. Northamptonshire ore (clay ironstone).
4. Black band ore, Mount Melville mine, St. Andrews.

One kilo. of each was reduced to fine powder, and 100 grams of Nos. 1, 2, and 3, and 500 grams of No. 4 were extracted with dilute hydrochloric acid as in the previous case. In each sample gallium was found, but the proportion was very small in the Northamptonshire ore, and still more minute in the black band. Without operating on several hundred grams it would have been scarcely possible to detect the gallium in the Mount Melville ore. These ores had not been roasted, and in this they differed from the sample received from the North Eastern Steel Works. The effect of roasting is the same as increasing the proportion of gallium in the ore.

Estimation of Gallium in the Blast Furnace Metal from Middlesbrough.

The sample weighing 575 grams consisted of 155 grams of fine powder and 420 grams of coarse powder. The latter portion was heated with hydrochloric acid until the acid was nearly neutralised, when the liquid was decanted and filtered.

Residue A. Solution B.

The *residue A* was heated with hydrochloric acid to which a small quantity of nitric acid was added from time to time; the solution was diluted and filtered.

Residue C. Solution D.

Residue C.—Dried and heated 0.5 gram in the oxyhydrogen flame. The lines of gallium, chromium, nickel, and iron are strong, and lines of sodium, manganese, potassium, copper, and silver are also present.

Solution B.—Boiled for two hours with part of the finely powdered sample added gradually to neutralise all the free acid, so that the gallium in the solution might be precipitated as a basic salt.* The solution was decanted and filtered. The residue was boiled with *solution D*, to which the remainder of the finely powdered sample was slowly added; after boiling for several hours the solution was filtered, and the *residue F* washed with water. The filtrate was mixed with that from *solution B*, the mixture forming *solution G*, which should be free from gallium. This solution was boiled with freshly precipitated copper hydrate,† and the precipitate examined spectrographically for gallium. It contained none.

Residue F.—Boiled with an excess of hydrochloric acid, diluted, filtered, and washed, *Residue H. Filtrate I.*

Residue H.—Dried, powdered, and mixed with *residue C*. Gently heated, the mixture decomposes and expels hydrocarbons, causing the mass to ignite and evolve some white fumes. The substance was thus seen to be very inflammable, and the temperature was reduced as quickly as possible. When cold, it was covered with aqua regia and heated on the water bath for several hours, then diluted and filtered. Filtrate added to *I*, forming *solution K. Residue L.*

Residue L.—A small quantity of it was heated in the oxyhydrogen flame. The gallium line is strong. 45 c.c. of strong sulphuric acid was heated in a porcelain basin until it gave off white fumes; the residue was then added forming a pasty mass which was kept hot for about three hours; white fumes being emitted during the whole time. Water was then added, and the liquid filtered. *Filtrate N. Residue M.* A portion of the latter was heated in the oxyhydrogen flame. The gallium line is still present.

Besides the small quantity remaining in the *residue M*, the gallium should now be in the *solutions K* and *N*. *Solution K* was evaporated nearly to dryness to expel the excess of acid, then diluted, saturated with sulphur dioxide, nearly neutralised with ammonia, and boiled to

* 'Comptes Rendus,' vol. 93, p. 818. See also a complete account, 'Séparation du Gallium d'avec les autres éléments,' par M. Lecocq de Boisbaudran. Paris, Gauthier-Villars. 1884. Reprinted from the 'Annales de Chimie,' 6. Série, t. 2.

† 'Comptes Rendus,' vol. 94, p. 1154.

reduce the iron to the ferrous state. This operation was unsuccessful, a quantity of iron remaining in the ferric state. The solution *N* was, therefore, added and the mixture evaporated that the more volatile acids might be expelled by the sulphuric acid. On adding water to the residue a small quantity of matter remains undissolved; it was removed by filtration. *Residue M₂*.

Up to this stage no reagent had been used which was likely to contain gallium, and we had to consider which of the processes known to separate gallium would be suitable under the conditions of our analysis. The simplest would have been to boil with iron or zinc, but gallium is found associated with both of these metals, and it was decided not to use them. Precipitation by barium carbonate would have been easily effected if sulphuric acid had not been present in such quantity. But, to avoid inaccuracy, the best—although more troublesome process—seemed to be the precipitation of the phosphates of the sesquioxide metals in an acetic acid solution, there being phosphoric acid already in the liquid. The precipitates should contain all the gallium, chromium, and aluminium as phosphates and some phosphate of iron. The gallium is easily separated from chromium and iron by fusion with caustic soda, and from phosphoric acid, aluminium, and chromium by precipitation with potassium ferrocyanide.

The iron was first reduced by passing sulphur dioxide into the solution until it became strongly charged, and heating to boiling, with addition of ammonia, to neutralise the excess of free acid. The addition of ammonia was continued until the white precipitate which formed remained undissolved after boiling for two or three minutes. Boiling water was then added to make the volume of the solution about four litres; this dilution caused a large quantity of light coloured precipitate to form. Ammonium acetate was added, and the liquid, after boiling for several minutes, filtered.

Residue O.—The filtrate was boiled and ammonium carbonate added until a quantity of pale, greenish-coloured precipitate was deposited. More ammonium acetate was added, and the liquid, still acid with acetic acid, was filtered. *Residue P*.

The process just described was repeated with the filtrate, the precipitate *R* being slightly darker than *P*. *Filtrate Q*.

Small quantities of the three residues, *O*, *P*, *R*, were examined spectrographically. The gallium lines are strongest in *R*. The filtrate *Q* was again boiled with addition of ammonium carbonate to neutralise some of the excess of acid, and the precipitate *S*, small in quantity and of a dark green colour, was removed by filtration. It contained only a trace of gallium.

The precipitates *O* and *S*, containing a much larger proportion of iron than *P* and *R*, were dissolved in hydrochloric acid, and the

gallium, &c., precipitated, after reducing the iron to the ferrous state. *First precipitate, U.* The second contained some gallium; the third, very dark in colour, was free from that metal.

The *precipitates P, R, and U* were dissolved in hydrochloric acid, and the solutions filtered to remove a small quantity of insoluble matter which was added to *residue M*. Two drops of violet-coloured filtrate were tested with potassium ferrocyanide, and so marked was the reaction that it was decided to repeat the process of reduction and precipitation to remove as much iron as possible. The first *precipitate W* contained nearly all the gallium; the second contained a small quantity, and the third contained none.

The first and second precipitations *U*, whose spectra are seen in 134¹ and 134³, contain a small quantity of gallium. They were redissolved, reduced, and boiled with excess of ammonium acetate, and the precipitate collected. A second precipitate was free from gallium. The former was fused with caustic soda, extracted with water and filtered. The filtrate was acidified with hydrochloric acid, and boiled with ammonia for some time, and the gallium phosphate thus precipitated was collected. This precipitate was added to *W*.

Residue W, &c.—This contained principally gallium and chromium phosphate with some iron phosphate. It was dissolved in hydrochloric acid, and the solution made to contain about one-fourth its volume of strong hydrochloric acid. Potassium ferrocyanide was added, but not an excess, and the bulky precipitate collected. An excess of the reagent was added to the filtrate, which, after standing twenty-four hours, was filtered. Very small quantities of the two precipitates were examined spectrographically; the second is decidedly richer in gallium than the first.

Residues M and M² with the small Residues added to them as described.—Ignited at a red heat to burn combustible matter. The mass became grey and weighed, when cold, 8 grams. It was very bulky, and consisted largely of silica. Fusion with fusion mixtures converted the silica into alkaline silicates, which were removed by solution in water, leaving a black residue. This was fused with caustic soda and sufficient nitre to oxidise the graphite, &c. Water dissolved all of this, excepting a small quantity of red oxide of iron, part of which was examined for gallium. None present.

The filtrate was acidified with hydrochloric acid, evaporated to dryness, and dried at 120° C. to dehydrate silicic acid.

The dry residue was digested with strong hydrochloric acid, and water added. It was then filtered to remove some silica, which was found to have retained only a trace of gallium.

The filtrate was mixed with a small excess of ammonia, and boiled for some time; the gallium being precipitated probably as phosphate. The filtrate in this and in all similar cases was again boiled, after

adding a few drops of ammonia; if any precipitate was produced it was collected and added to the other precipitate. The precipitate in this case was added to ferrocyanide precipitates obtained from the *residue W*. The paper, after being scraped to remove the residue as far as possible, was burnt in the oxyhydrogen flame. The gallium lines are strong.

The ferrocyanide precipitates with others rich in gallium were ignited at low redness to decompose the cyanides, and then fused with pure caustic soda. The product was extracted with water and filtered.

Residue from Fusion.—Dissolved in hydrochloric acid, expelled the excess of acid, added water, reduced the ferric salt, and filtered. Residue remaining contained only a trace of gallium.

Filtrate.—Boiled with an excess of ammonium acetate and filtered off the precipitate. The filtrate was mixed with sodium phosphate and boiled, thus yielding a second precipitate. The filtrate from this was again boiled, and ammonium carbonate added until a third precipitate was produced. Very small portions of these three precipitates were burnt in the oxyhydrogen flame. The first two were rich in gallium, while the third contained only a trace. Ignited the first and second precipitates, heated the residue in a platinum crucible with hydrochloric and sulphuric acids, expelled the former acid by heating until the white fumes of sulphuric acid were evolved, and then fused the residue with caustic soda. Extracted with water and filtered. After a second fusion the residue was practically free from gallium. The alkaline filtrates were acidified with hydrochloric acid, and the gallium precipitated by boiling with ammonia until the excess of ammonia was expelled. Filtered and tested the filtrate by repeating the process of boiling with ammonia until no further precipitate resulted.

The precipitates of gallium hydrate and phosphate, obtained as described, were dissolved in hydrochloric acid and, after adding one-fourth the volume of the solution of strong hydrochloric acid, an excess of potassium ferrocyanide was added. After standing for one day the precipitate was collected, washed, and ignited. It weighed 0.0670 gram.

This residue was dissolved by heating with strong sulphuric acid in a platinum crucible, some water being added, after heating strongly, and then an excess of caustic soda prepared from sodium. The crucible was then heated until the water was expelled, and the residue retained in the fused caustic soda. The process was repeated on the residue which remained after adding water and filtering. The second residue was practically free from gallium.

The filtrates were collected in a platinum basin, made faintly acid with hydrochloric acid, and saturated with sulphuretted hydrogen.

A brownish coloured precipitate was removed by filtration. It contained copper, lead, and silver, but no gallium. The filtrate was boiled to expel sulphuretted hydrogen, and the gallium precipitated with ammonia as described above. The precipitate was collected and ignited. It weighed 0.0288 gram.

This residue possessed a very light yellow colour. One milligram was burnt in the oxyhydrogen flame; its spectrum shows the two gallium lines very strongly. Lines of sodium, potassium, iron, calcium, and lead are present, but those of the last three are exceedingly weak.

The remaining 0.0278 gram of residue was fused with hydrogen potassium sulphate; water and sulphuric acid were added, and the crucible heated until fumes of sulphuric acid were evolved. Water was again added, and a small residue removed by filtration. This residue weighed 0.0040 gram.

The gallium was separated from the iron by two extractions with caustic soda solution. The ferric hydrate was dissolved in hydrochloric acid, and reprecipitated by ammonia. The ferric oxide weighed 0.0022 gram.

The gallium in the filtrate was then reprecipitated and weighed as oxide. It weighed 0.0213 gram.

A few drops of sodium phosphate were added to the filtrate, and sufficient ammonia to make it turn red litmus paper blue. After boiling for a few minutes the liquid was filtered, the paper being dried and burnt in the oxyhydrogen flame. The gallium lines are present in its spectrum, but are very weak.

The oxide of gallium now possessed a scarcely perceptible, faint yellow colour. It does not represent the whole of the gallium present in the sample, as a small quantity was removed and lost in testing the precipitations and residues. We are able, however, to estimate this quantity by comparing the lines in the different spectra with lines and spectra obtained by heating weighed quantities of gallium oxide. In this way we estimate the total quantity of gallium to be as follows:—

	Ga ₂ O ₃ .
Pure oxide	0.0213 gram.
In 0.001 gram of impure oxide....	0.0008 ,,
In residue insoluble in HKSO ₄	0.0004 ,,
In other substances.....	0.001 ,,

Ga₂O₃, total 0.0235 gram.

0.0235 gram of pure Ga₂O₃ contains 0.0175 gram of gallium,

$$\text{equal to } \frac{0.0175 \times 100}{575} = 0.00304 \text{ per cent.}$$

One part of gallium is contained in 33,000 parts of crude iron.

An estimation of the gallium in the "mixer metal" had been attempted in the spring of this year, but the separation was not as complete as in the process just described. The figure obtained, however, is so closely in accord with the above that we will briefly describe the process and record the result.

The sample, weighing 340 grams, was boiled with hydrochloric acid until the latter was nearly neutralised; the solution was then decanted, and fresh acid added to the residue. When the acid ceased to have any marked action the whole liquid was filtered, and the *residue A* washed, dried, and treated separately for the separation of gallium.

Filtrate B.—From this filtrate gallium was precipitated by calcium carbonate, but phosphates and sesquioxide metals, including chromium, rendered the precipitate a too complex mixture, and we had recourse to the ferrocyanide method.

The gallium was separated from the iron by pure sodium hydrate, and finally precipitated as hydrate and ignited. The oxide weighed 0.0149 gram, and this amount corresponds to 0.0033 per cent. of gallium in the sample or one part in 30,000 of the iron.

We know, by the spectrographic examination of the residues, &c., that the whole of the gallium was not obtained, and that the oxide weighed was not quite pure gallium oxide, but with the experience gained in this estimation we were able to make the more exact analysis already described.

In conclusion, we may state that this blast furnace metal contains more gallium than the richest source of that element hitherto known. The mineral referred to is a zinc blende from Bensburg on the Rhine, about eight miles from Cologne; it is found in the Franzisca adit of the Lüdérich mine. MM. Lecocq de Boisbaudran and Jungfleisch extracted 62 grams of crude gallium from 4300 kilograms, or nearly $4\frac{1}{4}$ tons of the ore; this is in the proportion of 1 in 72,000, but they believed the actual quantity present to be about 1 part of gallium in 50,000 of the ore.

We have recently discovered other sources of gallium, but cannot include the details of our later work in the present communication.

January 21, 1897.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Right Hon. Sir John Eldon Gorst, a member of Her Majesty's Most Honourable Privy Council, was admitted into the Society.

The following Papers were read :—

- I. "On *Cheirostrobos*, a new Type of Fossil Cone from the Calcareous Sandstones." By D. H. SCOTT, M.A., Ph.D., F.R.S.
- II. "Experiments in Examination of the Peripheral Distribution of the Fibres of the Posterior Roots of some Spinal Nerves. Part II." By C. S. SHERRINGTON, M.D., F.R.S., Holt Professor of Physiology, University College, Liverpool.
- III. "Cataleptoid Reflexes in the Monkey." By C. S. SHERRINGTON, M.D., F.R.S., Holt Professor of Physiology, University College, Liverpool.
- IV. "On Reciprocal Innervation of Antagonistic Muscles. Third Note." By C. S. SHERRINGTON, M.D., F.R.S., Holt Professor of Physiology, University College, Liverpool.

"Experiments in Examination of the Peripheral Distribution of the Fibres of the Posterior Roots of some Spinal Nerves. Part II." By C. S. SHERRINGTON, F.R.S., Holt Professor of Physiology, University College, Liverpool. Received November 12, 1896,—Read January 21, 1897.

(Abstract.)

This paper is in continuation of one brought before the Society in 1892, and published in 'Phil. Trans.,' B, vol. 184. In that communication the peripheral distribution of the sensory nerve-roots of the sacro-lumbar and the thoracic regions was examined. In the present the examination is extended to the cervical and brachial sensory

roots, and to the skin distribution of the cranial nerves. The communication is divided into four sections. In Section I the field of peripheral distribution of each root is described from the Vth cervical to the lower end of the brachial region. The description given is taken in each case from one particular experiment, which has proved a typical one for the root in question, and then deviations from this type are appended to it in the form of annotations. Particular attention was paid to the question of the skin-fields of the several divisions, ophthalmic, maxillary, and mandibular of the cranial Vth, in order to see if the fields possessed the characters of segmental skin-fields, or those of peripheral nerve-trunk skin-fields. They were found to conform with the latter, not with the former. A curious relation of the posterior edge of the field of the Vth to the external ear is found to exist, indicating that the position of the visceral cleft is still adhered to as a boundary line for the field of the trigeminus. The sense of taste as well as of touch is found to be destroyed in the anterior two-thirds of the tongue after intracranial section of the Vth; this makes it extremely doubtful whether the corda tympani can have gustatory functions in the monkey, as has been believed in some cases in man. No loss of eye-movements, or interference with them, has been found to result from intracranial section of the Vth.

The results obtained on the various successive nerve-roots cannot well be abstracted. The glossopharyngeal field on the tongue has been successfully delimited.

After cranial Vth and all the upper cervical posterior roots have been severed, there still persists a small field of sentient skin, which includes the external auditory meatus and a part of the pinna. This field, although not corresponding to the situation given by anthropotomists to the distribution of the auricular branch of the vagus, comes either from it or the glossopharyngeal. It presents interest as being the only field representing the whole cutaneous distribution of an entire nerve, which does not conform with the rules of zonal distribution holding good in the case of each of the other nerve-roots examined, and these now include the whole series. The posterior root of the Ist cervical nerve has a skin-field in the cat which includes the pinna. The posterior root of the same nerve in *Macacus* has no skin-field at all, its skin-field having apparently been included in the IIInd cervical of *Macacus*, not in the cranial Vth. The root fields contributing to the surface of the brachial limb are IIIrd, IVth, Vth, VIth, VIIth, and VIIIth cervical, and Ist, IIInd, and IIIrd thoracic. Of these, the VIIIth cervical is the only one which includes the whole of the surface of the free apex of the limb; its distribution in this respect closely resembles that of the VIth lumbar sensory root in the pelvic limb.

The IInd section of the communication deals with the degree of conformity between the distribution of the spinal ganglion fibres in the skin and their distribution in the underlying deep tissues of the limb. It is shown that, although the skin fields of the ganglia are in the middle of the limb region dislocated from the median line of the body, the distribution of the fibres of the root ganglion is nevertheless, when its deep distribution is taken into account, to a complete ray of tissue extending in an unbroken fashion from the median plane of the body out along the limb to (in the case of the nerves, extending farthest into the limb) the very apex of it. This distribution conforms, therefore, with that shown in a previous paper to be typical of the distribution of the ventral (motor) root. The distinction is not, therefore, as between afferent and efferent, but as between cutaneous and muscular. A detailed analysis of the distribution of the deep sensory fibres is in this paper carried out for the VIth lumbar spinal ganglion of *Macacus rhesus*; this ganglion was chosen because its skin-field, occupying the free apex of the lower limb, is one as far dislocated from the median line of the body as any in the whole spinal series, and presents, therefore, the greatest apparent discrepancy between the distribution of its afferent and efferent roots. A comparison of the distribution of the afferent and efferent roots in this (VIth lumbar) nerve was made by means of the Wallerian method; the results show the peripheral distribution of the two to be minutely similar. From this, and from other observations given, the rule is put forward as a definitely established one that the sensory nerves of a skeletal muscle in all cases derive from the spinal ganglion (or ganglia) corresponding segmentally with that (or those) containing the motor cells, whence issue motor nerve-fibres to the muscle. The reflex arc, in which the afferent and efferent nerve-cells innervating a muscle are components, need not, therefore, as far as anatomical composition is concerned, involve irradiation through more than a single spinal segment.

Section III deals with general features of arrangement recognisable in the distribution of the roots; for instance, the determination of the position of the primary dorsal and ventral lines of the limbs, the examination of the asserted rotation of the limbs and of the asserted torsion of the limbs, and of the asserted homologies between muscles, &c., of the brachial and pelvic limbs respectively, by the criteria for re-examination of such questions provided by the facts elicited in the course of the work; the cross-lapping of the skin-fields across the median line of the body, the overlapping of component parts of a single field, the serial overlapping of adjacent fields, the degree of overlapping in different regions of the body, the degree of overlapping in peripheral nerve-trunk fields, the amount of overlapping of spinal ganglion-fields compared with that of peripheral

nerve-trunks, the comparison of sensory overlapping with motor overlapping, the relation of overlapping to acuteness of sensation; individual variation, its extent and frequency, as far as can be judged from the skin-fields. Comparison between the human brachial plexus and that of *Macacus* is made, and it is pointed out that the human plexus is slightly prefixed, as compared with that of *Macacus*.

Finally, in Section IV, "shock," and various spinal reactions are examined, especially with reference to their effects upon the size and other features of the areas of the root-fields, &c., and the results collated and discussed.

"Cataleptoid Reflexes in the Monkey." By C. S. SHERRINGTON, M.A., M.D., F.R.S., Holt Professor of Physiology, University College, Liverpool. Received December 29, 1896,—Read January 21, 1897.

A phenomenon came under my observation in the course of experiments upon monkeys at the commencement of the present year which seems sufficiently interesting to merit record here. Its occurrence, so long as certain conditions of experiment are maintained, appears regular and predictable.

Although the character of the movements executed by the skeletal muscles when excited reflexly through the medium of the isolated spinal cord is variable, one feature common to them is their comparative brevity of duration. Many of them are, as pointed out by Fick and by Wundt years ago, hardly distinguishable in several particulars from the simple twitches elicitable from an excised muscle, so brief and local and inco-ordinate do they appear to be. Others are more prolonged, and, as I have described in a paper recently communicated to the Society, exhibit various forms of sequence or "march" (Hughlings Jackson). Without recapitulating the conclusions there drawn from the data given in that paper, I wish here to merely point out that of movements due to purely spinal reflex action, although some are fairly extensive, most are quite short-lasting, and not so prolonged as the longer of those that can be elicited under appropriate conditions from the cortex cerebri; also that if prolonged they, like the final phase of prolonged movements initiated from the cortex, tend to become clonic, or to exhibit that kind of action which in the paper referred to above I have designated "alternating."

The reflex movements, the subject proper of this note, are, on the contrary, of extremely prolonged duration, and absolutely devoid of clonic character and of alternating character. If the cerebral hemi-

spheres be carefully removed, *e.g.*, from a monkey, with avoidance of hæmorrhage and of fall of body temperature, and if sufficient time be allowed to elapse for subsidence in the animal of what may be called immediate shock, movements can be evoked remarkably different from those I have ever seen elicitable as purely spinal or as cerebral reactions. If a finger of one of the monkey's hands be stimulated, for instance, by dipping it into a cup of hot water, there results an extensive reflex reaction involving movement of the whole upper limb. The wrist is extended, the elbow flexed, the shoulder protracted, the upper arm being drawn forward and somewhat across the chest. The movement occurs after a variable and usually prolonged period of latent excitation. The movement, although it may be fairly rapid, strikes the observer each time as perfectly deliberate; it is of curiously steady and "smooth" performance. Sometimes it is carried out quite slowly, and then, as a rule, the extent of it is less ample. The most striking feature of the reflex is, however, that when the actual movement has been accomplished *the contraction of the muscles employed in it does not cease or become superseded by the action of another group, but is continued even for ten and twenty minutes at a time*. The new attitude assumed by the limb is maintained, and that too without clonus or even tremor. In the instance cited, namely, that of the fore limb, the posture assumed suggests the taking of a forward step in quadrupedal progression, and in that posture the animal will remain for a quarter of an hour at a time.

The degree of, for instance, flexion assumed in the new posture seems much dependent on the intensity and duration of the stimulus applied. If the degree is extreme, the attitude of the limb may not be maintained to its full extent for the time mentioned; thus, the elbow, at first fully flexed, will in the course of a minute or so be found to have opened somewhat. This opening can be often seen to occur *per saltum*, as it were, but the steps are quite small, and recurrent at unequal intervals of between perhaps a quarter of a minute and a minute. After some relaxation from the extreme phase of the posture has taken place, the less pronounced attitude, *e.g.*, semiflexion at the elbow, may persist without alteration obvious to inspection for ten minutes or more. Apart from the occasional step-like relaxations, the contraction of the muscles is so steady as to give an even line when registered by the myograph. A renewed stimulation of the finger excites further flexion, which is maintained as before in the way above described. The posture can be set aside without difficulty by taking hold of the limb and unbending it; the resistance felt in the process of so doing is slight; the posture thus broken down is not reassumed when the limb is then released.

Analogous results are obtainable on the hind limb. Hot water

applied to a toe evokes always, so far as I have seen, flexion of ankle and knee; usually of hip also. This movement is "deliberately" executed, and always institutes a maintained posture.

If finger (or toe) of both right and left limb be placed together in the hot water, there results symmetrical reflex movement of both the right and the left fore limbs (or hind limbs), leading to assumption of a fairly symmetrical posture by the right and left limbs respectively, the posture being similar to but duplicate of that evoked in the one limb only on excitation of that limb. This may appear a self-evident sequel to the observation given earlier, but is not so when an observation immediately to be mentioned is taken into consideration.

Not the least interesting part of the reflexes under consideration is a remarkable glimpse which they allow into the scope of reflex inhibition as regards the co-ordinate of movements of the limbs. Although the posture taken up by the right fore limb consequent upon excitation of a finger is symmetrically duplicated by the left limb when both hands are simultaneously stimulated, the effect of excitation of the two hands does not lead to symmetrical posture if the excitation be not synchronous but successive. If when the right arm has already assumed its posture in response to an excitation of the right hand, the left hand be stimulated, there results, while the left arm in obedience to the excitation is lifted and placed in the flexed posture, an immediate and, if the stimulus be at all more than slight, complete relaxation of the right arm. The right arm drops flaccid while the left is raised and maintained in the raised attitude. Similarly, excitation of the right foot breaks down the posture assumed by the right arm, and conversely, and even more easily, stimulation of the right hand breaks down a posture assumed by the right leg. Again, a nip of the right pinna causes relinquishment of a posture assumed by the right arm or by the right leg. If the right pinna is pinched when both arms are in this cataleptoid posture, complete inhibition can be readily exerted on the right arm, but usually only partial relinquishment can be induced in the left arm. To exert complete inhibition upon the posture of the left arm, the pinna pinched must be that of the left side. Similarly the posture reflexly evoked by appropriate stimulation of either hind limb can be inhibited by excitation of either pinna or of either fore limb, but predominantly by pinna and fore limb of the same side as the limb to be inhibited. The inhibition of the hind limb is much more easily elicited from the opposite hind limb than from the opposite fore limb or opposite ear. I have never yet seen it obtained diagonally upon the fore limb from the opposite hind limb.

The movements obtained in the limbs by exciting the limbs themselves are only cited above as examples to illustrate the general

characters of the condition. The details of the results will be given in a fuller paper dealing with the subject. I was prevented from inquiring thoroughly into the phenomenon when it was first met with; but in the course of the present summer and autumn the investigation has been systematically undertaken. I will conclude this preliminary note by adding that throughout the observations the animal's respiration remains apparently unaffected by the stimuli effective to produce the various reflexes and inhibitions such as above described. The respiration is tranquil, rather deep, regular, and often somewhat frequent. The animal in all my experiments has been completely blind, but a sharp conjunctival reflex exists. The knee jerks are elicitable but are not exaggerated. The tonus of the sphincters appears about normal. The pulse is full, regular, and fairly frequent.

I have not at present succeeded in evoking the cataleptoid reflex by simply placing the limb in the desired posture.

In applying the term cataleptoid to these reflexes, I do so because the reflexes recall, in some respects, strikingly certain phases of hypnotic condition, by some writers distinguished as cataleptic, and because the strict significance of the prefix implies a steady maintenance of possession subsequent to seizure, and is therefore peculiarly applicable here, whether these reflexes be or be not allied to hypnotic catalepsy.

“On Reciprocal Innervation of Antagonistic Muscles. Third Note.” By C. S. SHERRINGTON, M.A., M.D., F.R.S., Holt Professor of Physiology, University College, Liverpool. Received December 29, 1896,—Read January 21, 1897.

In a former number* of these ‘Proceedings’ attention was drawn to a particular form of correlation existing between the activity of antagonistic muscles. In it, one muscle of an antagonistic couple is, it was shown, relaxed in accompaniment with active contraction of its mechanical opponent. The instance then cited was afforded by certain of the extrinsic muscles of the eyeball, but I had previously noted indications of a like arrangement in studying the reflex actions affecting the muscles at the ankle-joint of the frog,† and it seemed probable that the kind of co-operative co-ordination demonstrated for the ocular muscles might be of extended application and occur in various motile regions of the body. The observations to be mentioned below do actually extend this kind of reciprocal innervation

* Vol. 52. April, 1893. Sherrington.

† Foster's ‘Journ. of Physiol.’ vol. 13, 1892.

to the muscles of antagonistic position acting about certain joints of the limbs.

If transection of the neural axis be carried out at the level of the *crura cerebri* in, *e.g.*, the cat, there usually ensues after a somewhat variable interval of time a tonic rigidity in certain groups of skeletal muscles, especially in those of the dorsal aspect of the neck and tail and of the extensor surfaces of the limbs. The details of this condition, although of some interest, it is unnecessary to describe here and now, except in so far as the extensors of the elbow and the knee are concerned. These latter affect the present subject. The extensors of the elbow and the knee are generally in strong contraction, but altogether without tremor and with no marked relaxations or exacerbations. On taking hold of the limbs and attempting to forcibly flex the elbow or knee a very considerable degree indeed of resistance is experienced, the *triceps brachii* and *quadriceps extensor cruris* become, under the stretch which the more or less effectual flexion puts upon them, still tenser than before, and on releasing the limb the joints spring back forthwith to their previous attitude of full extension. Despite, however, this powerful extensor rigidity, flexion of the elbow may be at once obtained with perfect facility by simply stimulating the toes or pad of the fore foot. When this is done the *triceps* enters into relaxation and the *biceps* passes into contraction. If, when the reflex is evolved, the condition of the *triceps* muscle is carefully examined, its contraction is found to undergo inhibition, and its tenseness to be broken down synchronously with and indeed very often accurately at the very moment of onset of reflex contraction in the opponent prebrachial muscles. The guidance of the flexion movement of the forearm may therefore be likened to that used in driving a pair of horses under harness. The reaction can be initiated in more ways than one, electrical excitation of a digital nerve or mechanical excitation of the sensory root of any of the upper cervical nerves may be employed; I have seen on one occasion a rubbing of the skin of the cheek of the same side effective.

Similarly in the case of the hind limb. The extensor muscles of the knee exhibit strong steady non-tremulent contraction under the appropriate conditions of experiment. Passive flexion of the knee can only be performed with use of very considerable force, the *quadriceps* becoming tight as a stretched string. The application of hot water to the hind foot then elicits, nevertheless, an immediate flexion at knee and hip, during which not only are the flexors of those joints thrown into contraction, but the extensors of the knee joint are simultaneously relaxed. Electric excitation of a digital nerve or of the internal saphenous nerve anywhere along its course will also initiate the reflex.

The same relaxation of existing contraction in the extensors can

be obtained by electrical excitation of the tract in the crura cerebri, when, as sometimes happens, that excitation evokes flexion at elbow or at knee. This and the previous fact which evidences that the result is obtainable after complete removal of the whole cerebrum bear out the view arrived at in my former paper that for this reciprocal and, as I believe, elementary co-ordination, it is not essential that "high level" centres (Hugblings Jackson) be employed. I incline to think, however, that this kind of co-ordination at elbow and knee is probably largely made use of in movements initiated *viâ* the cerebral hemispheres as well as in the lower reflexes, on the observation of which the present Note is based. This conclusion is indicated by its occurring in response to excitation of the pyramidal fibres in the crura. In the case of the reciprocal innervation of antagonistic ocular muscles I was able to prove that it took place even in "willed movements." It seems, in view of what has been shown above, legitimate to extend that result to the additional examples afforded by elbow-joint and knee.

Regarding the innervation of the *triceps brachii* and *quadriceps extensor cruris*, it is interesting to note that these muscles, which are of all among the limb muscles particularly difficult to provoke to action by local spinal reflexes, are the very ones which, when the level of the transection is pontial or prepontial, exhibit tonic contraction the most markedly. The well-known and oft-corroborated Sanders-Ezn phenomenon of inaccessibility of the extensors of the knee to spinal reflex action has, as I have recently shown, certain limitations, but at the same time so long as the transection is spinal—even when carried out so as to isolate not merely a portion of, but the whole, spinal cord entire from bulb to filum terminale—does apply very strictly to excitations arising in its own local region proper. And the spinal reflex relations of the *triceps brachii* in this respect, as pointed out elsewhere, somewhat resemble those of the distal portion of the *quadriceps extensor* of the leg. Alteration of the site of transection from infrabulbar to suprabulbar levels works a curious change in this. The Sanders-Ezn phenomenon then becomes subject to striking contravention. I have, after the higher transection, several times seen excitation of the hind foot itself provoke unilateral ideolateral extension of knee, a result incompatible with the Sanders-Ezn rule even under the limitations of ideolaterality, &c., which I consider must be attached to it. And similarly with the *triceps* at the elbow.

The difference between the accessibility of the *quadriceps* to reflex action after infrabulbar and after suprabulbar transection may, however, be less abrupt than it appears at first sight, and a superficial rather than a fundamental distinction. When *extensor rigidity* has ensued at elbow and knee after suprabulbar transection, the reflex excitability of *triceps brachii* and *quadriceps cruris* seems in a man-

ner as difficult as in the presence of exclusively spinal mechanisms. The reflex inhibitions the subject of this Note show, however, that the accessibility is not really greatly or even at all altered; the nexus is maintained, but the conduction across it is signalled by a different sign, *minus* instead of *plus*. The former, to find expression, must predicate an already existent quantity of contraction—*tonus*, to take effect upon. It seems likely enough that even when the transection is infrabulbar and merely spinal mechanisms remain in force, the same nexus obtains, but that then that background of tonic contraction is lacking, and that lacking the play of inhibitions remains invisible, never coming within the field of any ordinary method of observation.

Under the conditions adopted in my experiments, various other reflex actions, that seem probably examples of this same kind of co-ordination, can be studied, for instance, a sudden depression and curving downward of the stiffly elevated and tonically up-curved tail which can be elicited by a touch upon the perineum. But with these and also with other details regarding the reflexes at elbow and knee I hope to deal more fully in a paper to which the experiments recorded here are contributory.

“On *Cheirostrobos*, a new Type of Fossil Cone from the Calcareous Sandstone.” By D. H. SCOTT, M.A., Ph.D., F.R.S., Hon. Keeper of the Jodrell Laboratory, Royal Gardens, Kew. Received December 29, 1896—Read January 21, 1897.

The Peduncle.

The first indication of the existence of the remarkable type of fructification about to be described, was afforded by the study of a specimen in the Williamson collection, from the well-known fossiliferous deposit at Pettycur, near Burntisland, belonging to the Calcareous Sandstone Series at the base of the Carboniferous formation. This specimen is a fragment of stem, of which seven sections are preserved in the collection.* Its discoverer thought it might possibly belong to the *Lepidostrobos* found in the same bed. “If so,” he adds, “it has been part of the axis of a somewhat larger strobilus than those described.”†

A detailed examination of the structure of this specimen convinced me that it is essentially different from any *Lepidodendroid* axis, and is, certainly, a new type of stem.‡

* The cabinet-numbers are 539—545.

† Williamson, “Organisation of the Fossil Plants of the Coal-measures.” Part III. ‘Phil. Trans.’ 1872, p. 297.

‡ A short account of this specimen was given by me before the Botanical Section of the British Association at the Liverpool meeting, 1896.

As it was the examination of this fragment of stem which first put me on to the track of the new cone, it may be well shortly to describe its chief characteristics, reserving all details for a future paper.

The specimen, which is about 7 mm. in diameter, bears the bases only of somewhat crowded leaves, the arrangement of which, though not quite clear, was most probably verticillate, with from nine to twelve leaves in a whorl, those of successive whorls being superposed. Each leaf-base consists of a superior and an inferior lobe, and each lobe is palmately subdivided into two or three segments.

The leaf-traces, which are single bundles where they leave the central cylinder, subdivide in both planes on their way through the cortex, to supply the lobes and segments of the leaf.

The central cylinder is polyarch, the strand of wood having from nine to twelve prominent angles, with phloëm occupying the furrows between them. With the exception of the spiral protoxylem-elements at the angles, the tracheæ have multiseriate bordered pits, thus differing conspicuously from the scalariform tracheæ of the *Lepidodendrea*. The interior of the stele is occupied by tracheæ intermingled with conjunctive parenchyma. There is a well-marked formation of secondary tissues by means of a normal cambium.*

The Strobilus.

Mr. R. Kidston, F.G.S., kindly informed me that he had in his possession sections of a fossil cone from Burntisland having certain points in common with the Williamson specimen. On inspecting these sections with Mr. Kidston I was soon convinced that this undescribed cone really belonged to the same plant as the fragment of stem in the Williamson collection, and that the latter might well be the peduncle of the former. At the same time, I satisfied myself, and Mr. Kidston agreed with me, that the whole organisation of his cone is fundamentally different from that of any *Lepidostrobos*, the decisive point being that the new cone has compound branched sporo-

* The general structure of this axis, including the course of the bundles and the subdivision of the bracts, is correctly described by Williamson, *loc. cit.*, p. 297. As regards the latter point, he says "peripherally the bark breaks up into main or primary bracts, which again subdivide, as in the transverse section, into secondary ones, demonstrating that each primary bract does not merely dichotomize, but subdivides, both horizontally and vertically, into a cluster of bracts—a condition corresponding with what I have already observed in the smaller strobili described." These smaller strobili are those of the Burntisland *Lepidostrobos*, to which, by a strange coincidence, Williamson, *loc. cit.*, p. 295, erroneously attributed the same character, as regards subdivision of the bracts, which actually exists in the new cone. The only explanation appears to be, that Williamson interpreted the structure of the *Lepidostrobos* in the light of that of the peduncle, which, as we shall see, really belonged to a totally different fructification.

phylls, each of which bears a number of sporangia. It became evident that this cone must be placed in a new genus, and the conclusion arrived at from the study of the peduncle was thus confirmed.

Mr. Kidston most generously handed over his sections to me for examination and description, and also obtained for me from the owner the remains of the original block, from which I have had a number of additional sections prepared.

Only a single specimen of the cone is at present known. Before cutting sections, the piece, which includes the base but not the apex of the strobilus, was about 2 inches long. It was found at Pettycur, near Burntisland, in 1883, by Mr. James Bennie of Edinburgh. The specimen is calcified, and its preservation is remarkably perfect, so that the whole structure is well shown, though the complexity of its organisation renders the interpretation in some respects difficult.

The cone in its present somewhat flattened condition measures about 5 cm. by 2·3 cm. in diameter. The diameter in its natural state would have been at least 3·5 cm. That of the axis is about 7 mm., exactly the same as that of Williamson's peduncle. Thus the extreme length of the sporophylls, which have on the whole an approximately horizontal course, is about 1·4 cm.

The sporophylls are arranged in somewhat crowded verticils, fourteen of which were counted in a length of an inch, 2·5 cm. There are twelve leaves in each whorl, and the members of successive whorls are accurately superposed, a fact which is shown with the greatest clearness in tangential sections of the cone. This is evidently a point of great significance in considering the affinities of the fossil.

The sporophylls themselves have a remarkably complex form. Each sporophyll at its insertion on the axis, consists of a short basal portion or phyllopodium; the bases of the sporophylls belonging to the same verticil are coherent. The sporophyll branches immediately above its base, dividing into a superior and an inferior lobe, which lie directly one above the other in the same radial plane. Almost at the same point, each of the lobes subdivides in a palmate manner into three segments, which assume a horizontal course, whereas the common phyllopodium has an upward inclination. It is probable that sometimes, especially at the base of the cone, there may be two instead of three segments to each lobe. As a rule, however, each sporophyll consists of six segments, of which three belong to the superior (ventral or posterior) and three to the inferior (dorsal or anterior) lobe.

The segments are of two kinds—sterile and fertile. Both alike consist of a long, straight, slender pedicel, running out horizontally, and terminating at the distal end in a thick laminar expansion. The sterile segments are the longer, and in each the lamina bears an

upturned foliaceous scale as well as a shorter and stouter downward prolongation.

Each of the fertile segments ends in a fleshy laminar enlargement not unlike the peltate scale of an *Equisetum* or a *Calamostachys*. These fertile laminae, which are protected on the exterior by the overlapping ends of the sterile segments, bear the sporangia. Four, perhaps in some cases five, sporangia are attached, by their ends remote from the axis, to the inner surface of the peltate fertile lamina. Each sporangium is connected with the lamina by a somewhat narrow neck of tissue into which a vascular bundle enters. The sporangia are of great length, and extend back along the pedicels until they nearly or quite reach the axis.

The sterile and fertile segments alternate regularly, one above the other, in the same vertical series. So much is evident, but the question which segments are fertile and which sterile, has presented great difficulties, owing to the fact that the same segment can scarcely ever be traced continuously throughout the whole of its long course, and that the pedicels of sterile and fertile segments present no constant distinctive characters. For reasons, however, which will be fully given in a subsequent paper, I think it highly probable that in each sporophyll the segments of the *lower* lobe are sterile, and those of the *upper* lobe fertile, constituting the sporangiophores.

The sporangia and pedicels are all packed closely together so as to form a continuous mass. The external surface of the cone was completely protected by its double investiture of fertile and sterile laminae.

The spores are well preserved in various parts of the cone, and, so far as this specimen shows, are all of one kind, their average diameter being 0.065 mm. At the base of the cone, where macrospores, if they existed, might naturally be looked for, the spores are of the same size as elsewhere. So far, then, there is no evidence of heterospory. The spores are considerably larger than the microspores of the *Lepidostrobus*. Those of the Burntisland *Lepidostrobus*, for example, are barely 0.02 mm. in diameter. The spores of our plant approach in size those of *Sphenophyllum Dawsoni*, or the microspores of *Calamostachys Casheana*.

The sporangial wall, as preserved, is only one cell in thickness; it bears no resemblance to the palisade-like layer which forms the wall of the sporangium in *Lepidostrobus*, but has the same structure as that of a *Calamostachys*.* The sporangial wall of *Sphenophyllum Dawsoni* is similar.

The anatomy of the axis of the cone agrees closely with that of

* See Weiss, "Steinkohlen-Calamarien," vol. 2, 1884, Plate XXIV, figs. 3, 4, and 5; Williamson and Scott, "Further Observations on the Organisation of the Fossil Plants of the Coal-measures," Part I, 'Phil. Trans.,' 1894, Pl. 81, fig. 31.

the peduncle above described, except for the absence of any secondary tissues. The wood has twelve prominent angles, at which the spiral tracheæ are situated, so its development was, no doubt, centripetal. The inner tracheæ have pitted walls, and are intermixed with scattered parenchymatous cells, imperfectly preserved. The phloëm has entirely perished.

The most interesting anatomical feature is the course of the leaf-trace bundles, which can be followed with the greatest exactness on comparing sections in the three directions.

A single vascular bundle starts from each angle of the stele for each sporophyll, and passes obliquely upwards. When less than half way through the cortex, the trace divides into three bundles, one median and two lateral. The lateral strands are not always both given off exactly at the same point. A little further out, the median bundle divides into two, which in this case lie in the same radial plane, so that one is anterior, and the other posterior. The median posterior bundle is the larger, and before leaving the cortex this, in its turn, divides into three. There are now six branches of the original leaf-trace, three anterior, and three posterior, which respectively supply the lower and upper lobes of the sporophyll. The three segments of the lower lobe are supplied by the two lateral bundles first given off, and by the anterior median bundle, while the upper segments receive the posterior median bundle and its two lateral branches. In the base of the sporophyll, all six bundles can be clearly seen, in tangential sections of the cone, three above and three below. As the segments become free, one bundle passes into each, and runs right through the pedicel to the lamina. In the fertile lamina the bundle subdivides, a branch diverging to the point of insertion of each sporangium.

One of the longitudinal sections passes through the base of the cone, so as to show part of the peduncle in connection with it. In this peduncle secondary wood is present, just as in the separate specimen belonging to the Williamson collection. Higher up in the axis of the cone, where the sporophylls begin to appear, the secondary wood dies out. This evidence materially confirms the conclusion that the Williamson peduncle really belongs to our strobilus.

Diagnosis.

It is evidently necessary to establish a new genus for the reception of this fossil; the generic name which I propose is *Cheirostrobis*, intended to suggest the *palmate* division of the sporophyll-lobes (*χειρ*, hand). The species may be appropriately named *Pettycurensis*, from the locality where the important deposit occurs, which has yielded this strobilus and so many other valuable specimens of

palæozoic vegetation. The diagnosis may provisionally run as follows:—

Cheirostrobos, gen. nov.

Cone consisting of a cylindrical axis, bearing numerous compound sporophylls, arranged in crowded many-membered verticils.

Sporophylls of successive verticils superposed.

Each sporophyll divided, nearly to its base, into an inferior and a superior lobe; lobes palmately subdivided into long segments, of which some (probably the inferior) are sterile, and others (probably the superior) fertile, each segment consisting of an elongated stalk bearing a terminal lamina.

Laminæ of sterile segments foliaceous; those of fertile segments (or sporangiophores) peltate.

Sporangia large, attached by their ends remote from the axis, to the peltate laminæ of the sporangiophores.

Sporangia on each sporangiophore, usually four.

Spores very numerous in each sporangium.

Wood of axis polyarch.

C. Pettycurensis, sp. nov.

Cone, 3—4 cm. in diameter, seated on a distinct peduncle. Sporophylls, twelve in each verticil.

Each sporophyll usually sexpartite, three segments belonging to the inferior, and three to the superior, lobe.

Sporangia densely crowded.

Spores about 0.065 mm. in diameter.

Horizon: Calciferos Sandstone Series.

Locality: Pettycur, near Burntisland, Scotland. Found by Mr. James Bennie, of Edinburgh.

Both generic and specific characters are manifestly subject to alteration, if other similar fossils should be discovered. In the mean time the above diagnoses are given, in order to facilitate identification.

Affinities.

Any full discussion of affinities must be reserved for the detailed memoir, which I hope to lay before the Royal Society in a short time. At present only a few suggestions will be offered.

The idea of a near relationship to *Lepidostrobus*—so specious at first sight—is negatived by accurate investigation. There may have been a certain resemblance in external habit, as there is in the naked-eye appearance of the sections, but this means nothing more than that the specimen is a large cone, with crowded sporophylls and radially elongated sporangia. The only real resemblance to *Lepidostrobus* is in the polyarch strand of primary wood, but even here the details, as, for example, the structure of the tracheæ, do not

agree. In other respects the differences from any *Lepidodendroid* fructification are as great as they can be.

I do not doubt that the genus with which *Cheirostrobos* has most in common is *Sphenophyllum*. The chief points of agreement are as follows.

1. The superposed foliar whorls. This certainly agrees with the vegetative parts of *Sphenophyllum*, and, according to Count Solms-Laubach, the superposition holds good for the bracts of the strobili also.*

2. The deeply divided palmatifid sporophylls agreeing with the leaves of various species of *Sphenophyllum*, e.g., *S. tenerrimum*.

3. The division of the sporophyll into a superior or ventral, and an inferior or dorsal, lobe, agreeing with the arrangement in *Sphenophyllum Dawsoni*, or *S. cuneifolium*, according to M. Zeiller's interpretation.†

4. The differentiation of the sporophyll into sterile segments (bracts) and fertile segments (sporangiophores). The comparison with *Sphenophyllum* is much strengthened if, as I believe to be the case, the segments of the inferior lobe in *Cheirostrobos* are sterile, and those of the superior lobe fertile.

5. The repeated subdivision of the leaf-trace vascular bundles, in passing through the cortex of the axis,‡ as in *Sphenophyllum Stephanense*.

6. The attachment of the sporangia to a laminar expansion at the distal end of the sporangiophore. As regards this point, comparison should be made with the *Bowmanites Römeri* of Count Solms-Laubach (*loc. cit.*).

7. The structure of the sporangial wall.

I think that the sum of these characters, to which others might be added, justifies the suggestion that *Cheirostrobos* may be provisionally placed in the same *phylum*, or main division, of Pteridophyta, with *Sphenophyllum*, though indications of possible affinities in other directions are not wanting, and will be discussed on another occasion.

Cheirostrobos, even more than *Sphenophyllum* itself, appears to combine Calamarian with Lycopodiaceous characters, and might reasonably be regarded as a highly specialised representative of an ancient group of plants which lay at the common base of these two series.

It appears likely that in *Cheirostrobos* one of those additional forms

* '*Bowmanites Römeri*, eine neue Sphenophylleen Fructification,' 1895, p. 242.

† 'Étude sur la constitution de l'appareil fructificative des Sphénophyllum.' 'Mém. de la Soc. Géol. de France, Paléontologie,' Mém. 11, 1893, p. 37.

‡ Cf. Renault, 'Cours de Botanique fossile,' vol. 2, Pl. 14, fig. 2; Pl. 15, fig. 3, vol. 4, p. 15.

of Palæozoic Cryptogams, allowing of comparison with *Sphenophyllum*, has actually been brought to light, the discovery of which Dr. Williamson and I ventured to anticipate at the close of our first joint memoir.*

January 28, 1897.

Sir JOSEPH LISTER, Bart., F.R.C.S., D.C.L., President, followed by Sir JOHN EVANS, K.C.B., Treasurer and Vice-President, in the Chair.

Mr. John Eliot and Dr. Edward Charles Stirling were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Treasurer offered the congratulations of the Society to the President on his elevation to the peerage.

The following Papers were read :—

- I. "On the Capacity and Residual Charge of Dielectrics as affected by Temperature and Time." By J. HOPKINSON, F.R.S., and E. WILSON.
- II. "On the Electrical Resistivity of Electrolytic Bismuth at Low Temperatures and in Magnetic Fields." By JAMES DEWAR, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution; and J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London.
- III. "On the Selective Conductivity exhibited by certain Polarising Substances." By JAGADIS CHUNDER BOSE, M.A., D.Sc., Professor of Physical Science, Presidency College, Calcutta. Communicated by Lord RAYLEIGH, F.R.S.

* Williamson and Scott, "Further Observations on the Organisation of the Fossil Plants of the Coal-measures," Part I, 'Phil. Trans.' B, 1894, p. 946.

"On the Capacity and Residual Charge of Dielectrics as affected by Temperature and Time." By J. HOPKINSON, F.R.S., and E. WILSON. Received December 15, 1896,—Read January 28, 1897.

(Abstract.)

The major portion of the experiments described in the paper have been made on window glass and ice. It is shown that for long times residual charge diminishes with rise of temperature in the case of glass, but for short times it increases both for glass and ice. The capacity of glass when measured for ordinary durations of time, such as $1/100$ th to $1/10$ th second, increases much with rise of temperature, but when measured for short periods, such as $1/10^6$ second, it does not sensibly increase. The difference is shown to be due to the residual charge, which comes out between $1/50,000$ th second and $1/100$ th second. The capacity of ice when measured for periods of $1/100$ th to $1/10$ th second increases both with rise of temperature, and with increase of time, its value is of the order of 80, but when measured for periods such as $1/10^6$ second, its value is less than 3. The difference again is due to residual charge coming out during short times. In the case of glass, conductivity has been observed at fairly high temperatures and after short times of electrification; it is found that the conductivity after $1/50,000$ th second electrification is much greater than after $1/10,000$ th, but for longer times is sensibly constant. Thus a continuity is shown between the conduction in dielectrics which exhibit residual charge and deviation from Maxwell's law and ordinary electrolytes.

PRSL 60

"On the Electrical Resistivity of Electrolytic Bismuth at Low Temperatures, and in Magnetic Fields." By JAMES DEWAR, M.A., LL.D., F.R.S., Fullerman Professor of Chemistry in the Royal Institution; and J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London. Received January 4,—Read January 28, 1897.

In a previous communication to the Royal Society we have pointed out the behaviour of electrolytically prepared bismuth when cooled to very low temperatures, and at the same time subjected to transverse magnetisation.* During the last summer we have extended these

* See 'Proc. Roy. Soc.' vol. 60, p. 72, 1896. "On the Electrical Resistivity of Bismuth at the Temperature of Liquid Air," by James Dewar and J. A. Fleming. See also 'Phil. Mag.', September, 1895, Dewar and Fleming "On the Variation in the Electrical Resistance of Bismuth when cooled to the Temperature of Solid Air."

observations, and completed them, as far as possible, by making measurements of the electrical resistance of a wire of pure bismuth, placed transversely to the direction of the field of an electromagnet, and at the same time subjected to the low temperature obtained by the use of liquid air.

Sir David Salomons was so kind as to lend us for some time his large electromagnet, which, in addition to giving a powerful field, is provided with the means of easily altering the interpolar distance of the pole pieces, and also for changing from one form of pole piece to another.

The form of the pole piece most frequently used was that of a truncated cone. The magnet was always excited by a constant current obtained from a constant potential circuit. To save the considerable labour of determining again and again the strength of the interpolar field, this was determined once for all, corresponding to various interpolar distances and a given exciting current. The field was measured by suddenly removing from it a small exploring coil of wire of known area, the same being connected to a standardised ballistic galvanometer.

By this means a curve was constructed which showed at once the axial interpolar field at the central point in terms of the interpolar distances, the magnetising current being kept constant. This curve proved, as was to be expected, to be nearly a rectangular hyperbola.

This being done the bismuth wire to be examined was formed into a narrow loop of a single turn, about 3 or 4 cm. in length, and the ends soldered to leading-in wires of copper. The loop was placed in a small glass vacuum vessel, with the plane of the loop perpendicular to the direction of the axial magnetic field of the magnet. The loop was placed at equal distances from the two pole pieces, and in a nearly uniform field of known strength.

The vacuum vessel was then filled up with either liquid air, a solution of solid carbonic acid in ether, or else simply with paraffin oil. In a fourth case the vacuum vessel was closed, and liquid air having been placed in it, this liquid was caused to boil under a reduced pressure of 25 mm., thus giving a temperature falling as low as -203°C . In another experiment the vacuum vessel was dispensed with, the bismuth wire was simply wrapped in cotton wool, placed between two pieces of thin mica between the pole pieces, and by pouring upon the wrapping a copious libation of liquid air, the temperature of the bismuth wire was reduced to -185°C .

In all cases great care was taken to avoid thermo-electric complications, by providing that the soldered junctions by which the bismuth wire is connected to the copper leading-in wire were at exactly the same temperature, and to secure this the junctions were always kept well covered with the refrigerating solution.

The bismuth employed was electrolytic bismuth pressed into wire 0.5245 mm. in diameter, and its purity was confirmed by spectroscopic examination.

These arrangements being made, the observations consisted in measuring the electrical resistance of the bismuth at one temperature, but when the transverse magnetic field had values varying from zero to nearly 22,000 C.G.S. units.

In the following tables the results are collected. The electrical resistivity of the bismuth is stated for each temperature, and for the various transverse fields employed.

As the specimens of the bismuth wire used in the various experiments had different lengths, the actual figures of observation are not given, but they have been reduced so as to give the volume resistivity of the bismuth, corresponding to a certain temperature and magnetic field strength.

In the case of the experiment in liquid air boiling under a reduced pressure, on account of the size of the vacuum vessel necessary to contain the required initial volume of liquid air, the pole pieces of the magnets could not be brought very near together, and hence the field could not be raised to a very high value.

Hartman and Braun's Pure Electrolytic Bismuth.

Resistivity of Bismuth Transversely Magnetised at Ordinary Temperatures (+19° C.).

Strength of field (C.G.S. units).	Volume resistivity in C.G.S. units.
0	116,200
1,375	118,200
2,750	123,000
8,800	149,200
14,150	186,200
21,800	257,000

Resistivity of Bismuth Transversely Magnetised at -79° C.

Strength of field (C.G.S. units).	Volume resistivity in C.G.S. units.
0	78,300
650	83,300
2,300	103,500
3,350	114,800
4,100	134,000
5,500	158,000
7,900	201,000
14,200	284,000

Resistivity of Bismuth Transversely Magnetised at -185°C .

Strength of field (C.G.S. units).	Volume resistivity in C.G.S. units.
0	41,000
1,375	103,300
2,750	191,500
8,800	738,000
14,150	1,730,000
21,800	6,190,000

*Hartman and Braun's Pure Electrolytic Bismuth.*Resistivity of Bismuth Transversely Magnetised at -203°C .

Strength of field (C.G.S. units).	Volume resistivity in C.G.S. units.
0	34,300
2,450	283,500

Electrical Resistivity of Bismuth in C.G.S. units, transversely magnetised in a Constant Magnetic Field, but at variable Temperatures.

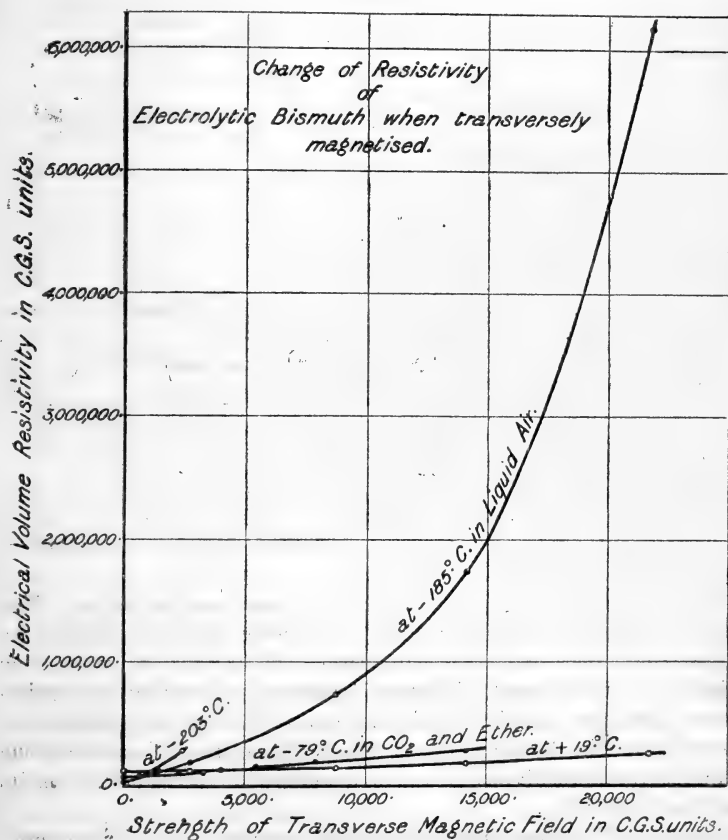
Temperature of the bismuth wire.	Out of the magnetic field.	In the magnetic field.		
		Strength 2450 C.G.S. units.	Strength 5500 C.G.S. units.	Strength 14,200 C.G.S. units.
+ 19°C .	116,200	123,500	132,000	187,000
- 79 "	78,300	105,000	153,000	284,000
-185 "	41,000	186,000	419,000	1,740,000
-203 "	34,300	283,500	—	—

It will be seen that the observations lead to the following conclusions. If the transverse field is zero, then cooling the bismuth always reduces its resistance. If then the bismuth is transversely magnetised, the resistance is increased, and for every temperature below the normal one (about 20°C .), there is some particular strength of transverse field, which just annuls the effect of the cooling, and brings the resistance of the bismuth back again to the same value it had when not cooled, and not in any magnetic field. Hence the curves showing the resistance at any temperature lower than the normal one (20°C .) as a function of the transverse field, cross the curve showing the resistance as a function of the field when taken at the normal temperature. These crossing points are, however, not identical for

different resistance-temperature-field curves. The lower the temperature the less is the strength of field which will bring the bismuth back to its original resistance when not cooled and not in the field.

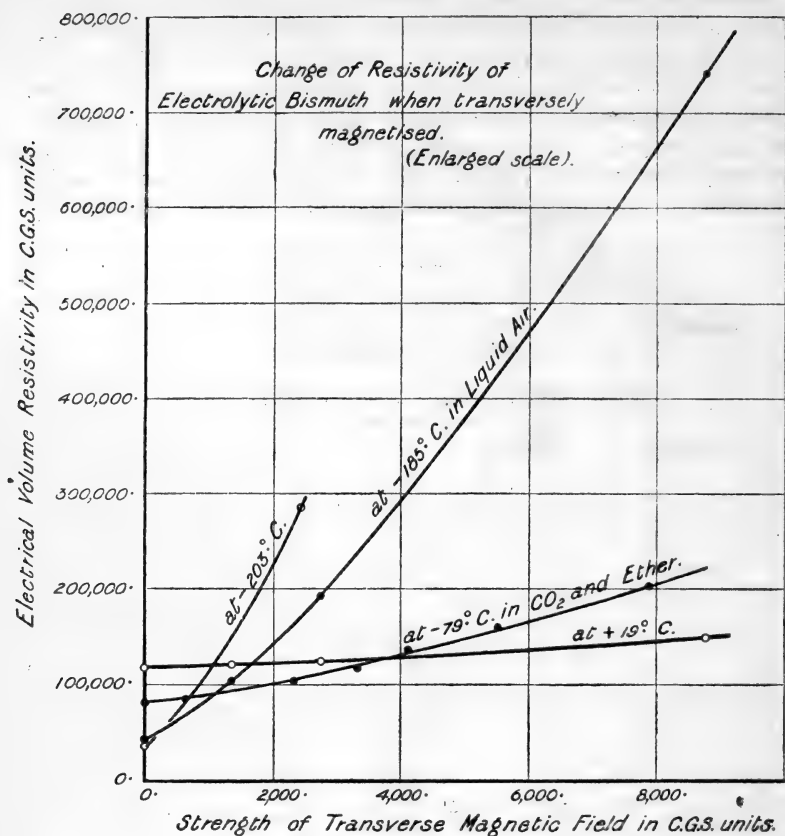
The observations have been set out graphically in the diagrams in figs. 1, 2, and 3, and it will be seen that there are in fig. 1 four curves. Each of these curves corresponds to a different temperature, viz., that of liquid air ($-185^{\circ}\text{C}.$), liquefying carbonic acid in ether ($-79^{\circ}\text{C}.$), ordinary temperatures ($20^{\circ}\text{C}.$), and a fourth shorter curve, which corresponds to a very low temperature of $-203^{\circ}\text{C}.$, obtained by

FIG. 1.



evaporating liquid air under a reduced pressure. This last curve is only continued for a short distance. These curves show the mode of variation of the resistance of the bismuth at a constant temperature as a function of the transverse magnetic field; and they show how

FIG. 2.



remarkably the resistance is affected by such magnetisation. The curve of resistance taken in liquid air, shows that by a transverse magnetising field having a strength of 22,000 C.G.S. units, the resistance of the bismuth is made 150 times greater than the resistance of the same wire in a zero field, but at the same temperature.

The lower the temperature to which the bismuth is reduced the greater is the multiplying power of a given transverse field upon its electrical resistivity.

Hence a still lower temperature than we have been able to apply would doubtless render the bismuth still more sensitive to transverse magnetisation.

We have already shown that pure bismuth is no exception to the generally observed fact that all pure metals continuously lose their electrical resistivity as they approach in temperature the absolute

zero. Hence at this last temperature it should be converted into a non-conductor by a sufficiently strong transverse magnetisation. This result will have to be taken into consideration in framing any theory of electrical conduction.

In this respect bismuth is a remarkable exception to other metals. We have tried the effect of transverse magnetisation at low temperatures on zinc, iron, and nickel, but find no effect sensibly greater at low than at ordinary temperatures, although these metals have their resistance affected by magnetisation to a small degree.

Bismuth has an exceptional position amongst other metals, both in respect of its large coefficient of the Hall effect, and also in the degree to which its resistance is thus affected by transverse magnetisation, and in addition, as above shown, in the degree to which cooling to low temperatures affects this ability to be so changed by magnetisation.

Very small amounts of impurity in the metal reduce these remarkable qualities considerably.

We may mention here that we have repeated the experiments we made some time ago* on certain specimens of chemically prepared bismuth, and for which we found the electrical resistance had a minimum value for a certain temperature. We have again verified this fact, both for the same and for a similar specimen. In the former experiments the bismuth wire used was embedded in paraffin wax during the cooling, and the suspicion had arisen that strains might thus have been produced which had affected the results. In the repetition of the experiments, we suspended the bismuth wire freely in liquid air, so that no strains could be produced; and, in addition, we tried the effect of mechanical stress on the resistance directly. We satisfied ourselves that the cause of the anomaly in the behaviour of the chemically prepared bismuth in respect of electrical resistance at low temperatures was not to be found in any effect due to strain.

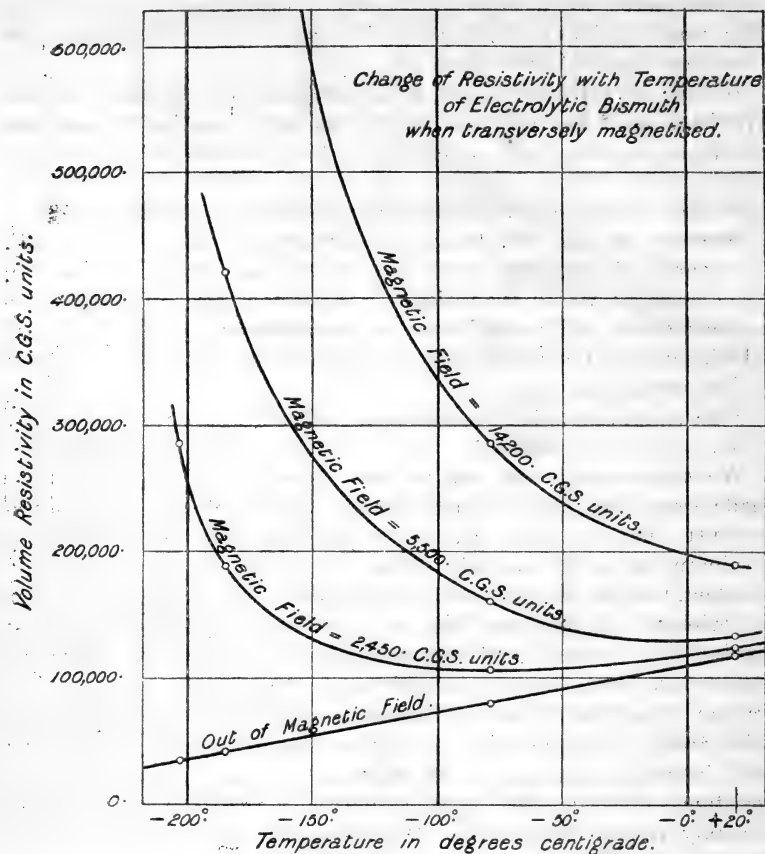
In fig. 3 a series of curves have been drawn showing the variation in resistivity of the electrolytic bismuth for certain constant transverse magnetic fields and varying temperatures. These curves were obtained by taking sections of the curves in figs. 1 and 2. The curves in fig. 3 are practically the continuation from 19° C. down to -186° C. of curves which have been given by Mr. J. B. Henderson,† for a range of temperature lying above 0° C.

They show that if a wire of electrolytic bismuth is placed transversely in a certain magnetic field, there is, for a wide range of field,

* See 'Phil. Mag.,' September, 1895, p. 303. Dewar and Fleming "On the Variation in the Electrical Resistance of Bismuth when cooled to the Temperature of Solid Air."

† See 'Phil. Mag.,' 1894, vol. 38, p. 488.

FIG. 3.



a certain temperature at which the bismuth has a minimum electrical resistivity, and, therefore, a zero temperature coefficient, and that the temperature of this turning point is higher the stronger the transverse field. These curves also show that at a temperature of about 150°C ., the bismuth would probably cease to have its resistivity affected by a transverse magnetic field.*

In conclusion, we desire to mention the assistance we have received from Mr. J. E. Petavel in the work described above.

* Drude and Nernst ('Wied. Ann.', vol. 42, p. 568) found that, with a transverse field of 7000 C.G.S. units, the total percentage increase of resistance of electrolytic bismuth was 22.0, 8.0, 1.0, and 0.4 per cent. respectively at temperatures of 16°C ., 100°C ., 223°C ., and 290°C .

"On the Selective Conductivity exhibited by certain Polarising Substances." By JAGADIS CHUNDER BOSE, M.A., D.Sc., Professor of Physical Science, Presidency College, Calcutta. Communicated by Lord RAYLEIGH, F.R.S. Received January 14,—Read January 28, 1897.

In my paper "On the Polarisation of Electric Rays by Double-refracting Crystals" (*vide* 'Journal of the Asiatic Society of Bengal,' May, 1895), and in a subsequent paper "On a New Electro-Polariscope" ('Electrician,' 27th December, 1895), I have given accounts of the polarising property of various substances. Amongst the most efficient polarisers may be mentioned nemalite and chrysotile. Nemalite is a fibrous variety of brucite. In its chemical composition it is a hydrate of magnesia, with a small quantity of protoxide of iron and carbonic acid. This substance is found to absorb very strongly electric vibrations parallel to its length, and transmit those that are perpendicular to the length. I shall distinguish the two directions as the directions of absorption and transmission. Chrysotile is a fibrous variety of serpentine. In chemical composition it is a hydrous silicate of magnesia. Like nemalite, it also exhibits selective absorption, though not to the same extent. The transmitted vibrations are perpendicular, and those absorbed parallel to the length. Different varieties of these substances exhibit the above property to a greater or less extent. I have recently obtained a specimen of chrysotile with a thickness of only 2·5 cm.; this piece completely polarises the transmitted electric ray by selective absorption.

The action of these substances on the electric ray is thus similar to that of tourmaline on light. It may be mentioned here that I found tourmaline to be an inefficient polariser of the electric ray; it does transmit the ordinary and the extraordinary rays with unequal intensities, but even a considerable thickness of it does not completely absorb one of the two rays.

In Hertz's polarising gratings, electric vibrations are transmitted perpendicular to the wires, the vibrations parallel to the wires being reflected or absorbed. Such gratings would be found to exhibit electric anisotropy, the conductivity in the direction of the wires being very much greater than the conductivity across the wires. The vibrations transmitted through the gratings are thus perpendicular to the direction of maximum conductivity—or parallel to the direction of greatest resistance. The vibration absorbed is parallel to the direction of maximum conductivity.

As the nemalite and chrysotile polarised the electric ray by unequal absorption in the two directions, I was led to investigate whether

they, too, exhibited unequal conductivities in the two directions of absorption and transmission.

Nemalite, unfortunately, is difficult to obtain, and the specimens I could get here were too small to make the necessary measurements. I have, however, in my possession two specimens which I brought from India; of these, one is a perfect specimen of a fair size, and I obtained with it strong polarisation effects. The second piece is not as good as the first, and rather small in size. I cut from this latter piece a square of uniform thickness, the adjacent sides of the square being parallel to the directions of transmission and absorption respectively. The resistances of equal lengths in the two directions (with the same cross section) were now measured.

The first specimen I gave to Messrs. Elliott Brothers for measurement. They informed me, on the 13th of October last, that the resistance in the direction of transmission was found to be 35,000 megohms, and that in the direction of absorption, only 14,000 megohms.

It will thus be seen that the direction of absorption is also the direction of greatest conductivity, and the direction of transmission is the direction of least conductivity.

My anticipations being thus verified, I proceeded to make further measurements with other specimens. From the perfect specimen of nemalite in my possession, I cut two square pieces, A and B. The size of piece A is 2.56×2.56 cm., with a thickness of 1.1 cm. B is $2.76 \times 2.76 \times 1.2$ cm.

For the determination of resistances I used a sensitive Kelvin galvanometer, having a resistance of 7000 ohms. With three Leclanché cells, 1.4 volt each, and an interposed resistance equivalent to 55,524 megohms, a deflection of 1 division in the scale reading was obtained. The following table (p. 435) gives the results of the measurements which I carried out.

The results given clearly show how the difference of absorption in the two directions is related to the corresponding difference in conductivity.

I then proceeded to make measurements with chrysotile. The specimens I could obtain were not very good. I cut two from the same piece, and a third specimen was obtained from a different variety. The ratios of conductivities found in the three specimens were 1 : 10, 1 : 9, and 1 : 4 respectively. In every case the direction of absorption was found to be the direction of maximum conductivity.

[A fibrous variety of gypsum (CaSO_4), popularly known as Satinspar, also exhibits double absorption; and in this case, too, the conductivity in the direction of absorption is found to be very much greater than in that of transmission.]

Specimen A.	Deflections.	Resistance between two opposed faces 2.56×1.1 cm. separated by 2.56 cm.	Ratio of the conductivities.
In the direction of transmission	26	2136 megohms	} 1 : 13.8
" " absorption..	360	154 "	

Specimen B.	Deflections.	Resistance between two opposed faces 2.76×1.2 cm. separated by 2.76 cm.	Ratio of the conductivities.
In the direction of transmission	28	1983 megohms	} 1 : 13.4
" " absorption..	370	150 "	

One of the strongest polarising substances I have come across is the crystal epidote. The crystal is very small in size, and I could not get with it complete absorption of one of the two rays. But it exhibits very strong depolarisation effect, even with a thickness as small as 0.7 cm. This is, undoubtedly, due to strong selective absorption in one direction. I cut a square from this crystal 0.7×0.7 cm. with a thickness of 0.4 cm. Using an E.M.F. of 14 volts the deflections obtained (proportional to the two conductivities) were 105 and 20 divisions respectively. The conductivities in the two directions are, therefore, in the ratio of 5.2 : 1. With an E.M.F. of 100 volts and a diminished sensibility of the galvanometer, the deflections were 205 and 40, the ratio of the conductivities being as 5.1 : 1.—January 28, 1897.]

It would thus appear that substances like nemalite which polarise by double absorption, also exhibit double conductivity. It is probable that, owing to this difference of conductivity in the two directions, each thin layer unequally absorbs the incident electric vibrations; and that by the cumulative effect of many such layers, the vibrations which are perpendicular to the direction of maximum conductivity are alone transmitted, the emergent beam being thus completely polarised.

[Owing to the great difficulty in obtaining suitable specimens, I have not been able to make a more extended series of determinations. The relation found, in the cases described above, between double absorption and double conductivity is, however, suggestive.

It should, however, be borne in mind that the selective absorption exhibited by a substance depends, also, on the vibration frequency of the incident radiation. I have drawn attention to the peculiarity of tourmaline which does not exhibit double absorption of the electric ray to a very great extent. The specimen I experimented with is, however, one of a black variety of tourmaline, and not of the semi-transparent kind generally used for optical work.

Though the experiments already described are not sufficiently numerous for drawing a general conclusion as to the connection between double absorption attended with polarisation, and double conductivity, there is, however, a large number of experiments I have carried out which seem to show that a double-conducting structure does, as a rule, exhibit double absorption and consequent polarisation. Out of these experiments I shall here mention one which may prove interesting. Observing that an ordinary book is unequally conducting in the two directions—parallel to and across the pages—I interposed it, with its edge at 45° , between the crossed polariser and analyzer of an electropolariscope. The extinguished field of radiation was immediately restored. I then arranged both the polariser and the analyzer vertical and parallel, and interposed the book with its edge parallel to the direction of electric vibration. The radiation was found completely absorbed by the book, and there was not the slightest action on the receiver. On holding the book with its edge at right angles to the electric vibration, the electric ray was found copiously transmitted. An ordinary book would thus serve as a perfect polariser of the electric ray. The vibrations parallel to the pages are completely absorbed, and those at right angles transmitted in a perfectly polarised condition.—*January 28, 1897.*]

February 4, 1897.

Sir JOSEPH LISTER, Bart., F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President stated that a paper had been received from Dr. Arthur Willey, Balfour Student of the University of Cambridge, the recipient of a Government Grant, and now staying at the Loyalty Islands, to the effect that he had discovered the ova of Nautilus.

The following Papers were read:—

- I. "On the Condition in which Fats are absorbed from the Intestine." By B. MOORE and D. P. ROCKWOOD. Communicated by Professor E. A. SCHÄFER, F.R.S.
- II. "The Gaseous Constituents of certain Mineral Substances and Natural Waters." By WILLIAM RAMSAY, F.R.S., and MORRIS W. TRAVERS, B.Sc.
- III. "Some Experiments on Helium." By MORRIS W. TRAVERS, B.Sc. Communicated by Professor W. RAMSAY, F.R.S.
- IV. "On the Gases enclosed in Crystalline Rocks and Minerals." By W. A. TILDEN, D.Sc., F.R.S.
- V. "On Lunar Periodicities in Earthquake Frequency." By C. G. KNOTT, D.Sc., Lecturer on Applied Mathematics, Edinburgh University (formerly Professor of Physics, Imperial University, Japan). Communicated by JOHN MILNE, F.R.S.

“On the Condition in which Fats are absorbed from the Intestine.” By B. MOORE and D. P. ROCKWOOD. Communicated by Professor E. A. SCHÄFER, F.R.S. Received December 24, 1896,—Read February 4, 1897.

(From the Physiological Laboratory of University College, London.)

In 1858 Dr. W. Marcet* announced to this Society the discovery that bile possesses the remarkable property of dissolving to a clear solution large amounts of fatty acids, and mixtures of these, when heated above their melting points, and that, on cooling, these bodies are again thrown out as a fine precipitate or emulsion.

We have repeated these experiments, and are able to confirm the accuracy of Marcet's observation. Thus we found that 6 c.c. of dog's bile at 62° C. dissolved completely 1·5 grams of the mixed fatty acids† of beef suet, and similar solubilities were found in other cases.

No other observations than these have, so far as we are aware, been made on the effect of temperature on the solubility of fatty acids in bile; although different writers have mentioned that fatty acids are soluble in bile, no measurements have been made of the extent of their solubility. Altmann‡ has recently surmised that fats are absorbed from the intestine as fatty acids, dissolved in the intestine by the agency of the bile, but has made no quantitative experiments on the solubilities of the fatty acids in bile. The forgotten experiments of Marcet, mentioned above, led us to think that the fatty acids might possess, *at the temperature of the body*, a fair amount of solubility in bile, and as the solubility at this temperature is that of most physiological interest, we have made a series of determinations of the solubilities of oleic, palmitic, and stearic acids, and of natural mixtures of these in the proportions in which they occur in lard, beef suet, and mutton suet, in the bile of the ox, pig, and dog.

Different methods were used in the determination of these solubilities:—

1. To a measured amount of the bile under experiment, kept at a temperature of 39° C., small weighed quantities of the fatty acid under experiment were added, until no more dissolved.

2. A quantity of bile was saturated at 39° C., with excess of the fatty acid, and filtered from the excess of undissolved acid through a

* ‘Roy. Soc. Proc.,’ 1858, vol. 9, p. 306.

† Throughout this communication the expression “fatty acids” means the fatty acids present in fats, oleic, palmitic, and stearic acids.

‡ ‘Arch. f. Anat. u. Physiol.,’ 1889, Anat.-Abth., Suppl. Band, p. 86.

hot funnel, at this temperature; the filtrate was cooled to about 0° C., and the precipitate collected, dissolved in ether, recovered therefrom, and weighed; the weight, compared with the volume of the filtrate, gave a measure of the solubility.

3. To a series of equal volumes (10 c.c.) of bile in test-tubes, a rising series of weights of fatty acids was added (0·05, 0·1, 0·15, 0·2, &c., grams), and those tubes noted, in which, after the lapse of a sufficient time at 39° C., complete solution did not take place.

The following is a summary of our results.

	Lard fatty acids.*	Beef suet acids.	Mutton suet acids.	Oleic acid.	Palmitic and stearic acids.
1. Ox bile	2·5—4 p. c.	2·5—3 p. c.	1—2·5 p. c.	4—5 p. c.	Less than 0·5 p. c.
2. Pig's bile ..	4 „	5—6 „	1—2·5 „	—	—
3. Dog's bile ..	6·25 „	4—7 „	2 „	—	—

The fatty acids are not dissolved as soaps, but probably as fatty acids, for the solution becomes strongly acid; moreover, the material thrown out on cooling dissolves easily in ether, and, when recovered, saponifies at once with sodium carbonate. The solution is not entirely due to the bile salts, for mere removal of the “bile mucin” greatly diminishes the solvent power, although the “mucin” redissolved in sodium carbonate solution has no solvent power, and, again, a solution of mixed bile salts† stronger than bile has not nearly so much solvent power as the bile itself.

Palmitic and stearic acids are very feebly soluble in bile at 39° C., and in mixtures are probably dissolved by the aid of the admixed oleic acid.

Action of Filtered Intestinal Contents on Fats.

The filtered intestinal contents contain both pancreatic juice and bile, and hence should both decompose and dissolve fats at body temperature if these are absorbed as *dissolved* fatty acids; this was experimentally found to be the case with filtered intestinal contents of the dog, which in different cases possessed a very variable

* The numbers given are the minimum and maximum of a number of determinations in different samples of bile.

† The solution used was a 9 per cent. solution of the bile salts of a sample of ox bile which dissolved 2·5 per cent. of the fatty acids of beef suet; this solution of bile salts only dissolved 1 per cent.

power, dissolving 1 to 5 per cent. of the fat of beef suet at 39° C. The solution becomes viscid, semi-fluid, or completely solid on cooling, and redissolves on warming again. With the filtered contents of the intestine of the pig and rabbit similar results were *not* obtained, but the fat became altered, being in part converted into fatty acids, and in part giving rise to a voluminous precipitate.

Simultaneous Action of Pancreas and Bile on Fats.

Finely minced, fresh dog's pancreas (1 gram) was added to bile (10 c.c.), and then the fat of beef suet (0.25 gram); the fat completely dissolved in three hours at 40° C.; on cooling, the solution became turbid, and finally semi-solid. In a control experiment, pancreas alone decomposed fat into fatty acids, but did not dissolve it.

The solubilities stated above are quite sufficient to account for the removal of all the fat of the food from the intestine as dissolved fatty acid, since they exceed the concentrations found in the intestine of other materials, such as sugars and albumoses, which are removed in solution. Other experiments, however, on the reaction of the intestine during fat absorption, lead us to think that all the fat is not removed as dissolved fatty acids, but that these are replaced to a variable extent (*in some animals, to a very large extent or completely*) by dissolved soaps.

Reaction of Intestinal Contents during Fat Absorption.

We have determined the reaction of the contents of the dog's small intestine during fat absorption, from pylorus to cæcum, to various indicators, litmus, methyl-orange, and phenolphthaleïn; and cannot agree with the statement of some other experimenters, that it is acid throughout.* In sixteen experiments on this animal we only once found the reaction acid to litmus up to the cæcum, and this was an obviously poor experiment, in which the intestine was almost empty. The reaction to litmus at the pylorus is neutral, faintly acid, or faintly alkaline; from here onwards the acidity increases, reaches a maximum about the middle of the small intestine, and then becomes less acid, to change to alkaline at a point situate two-thirds to three-fourths of the way along the intestine; from this point on to the cæcum the alkalinity increases.† The reaction to methyl-orange and phenolphthaleïn explains this; the intestine is alkaline to methyl-orange all the way from pylorus to cæcum, and equally com-

* Cash, 'Arch. f. Anat. u. Physiol.,' 1881, p. 386; Munk, 'Zeitsch. f. Physiol. Chem.,' vol. 9, 1885, pp. 572, 574.

† There is usually a reversion to an acid reaction in the large intestine, in which case the contents of the cæcum are almost neutral.

pletely acid to phenolphthaleïn, showing that *the acid reaction to litmus in the upper part is due to weak organic acids*, while the alkaline reaction in the lower is due to fixed alkali, accompanied by dissolved carbonic acid. The alkaline reaction to methyl-orange in the upper part, where it is acid to litmus and phenolphthaleïn, shows that in that part there is an excess of bases, above that quantity necessary to combine with all the inorganic acids, which are combined with very weak organic acids (probably fatty acids), for methyl-orange is a stable indicator, and does not react to such acids, while litmus, and, still more so, phenolphthaleïn, are indicators which are affected by these acids. In the lower third or thereabouts, where the reaction is alkaline to litmus, there cannot be any fatty acids present in solution.

Any fat absorbed as free fatty acid in solution must, therefore, be taken up from the upper two-thirds or three-fourths of the intestine where the reaction is acid to litmus, but even here a considerable part is probably being absorbed in solution as soaps, as is shown by the reaction being at the same time alkaline to methyl-orange. In the lower part all the fat absorbed must be taken up as soaps.

During fat absorption in the white rat,* the reaction of the contents of the small intestine is commonly alkaline to litmus from pylorus to cæcum, and is never acid for a greater distance than 2 or 3 in. below the pylorus; in this animal, therefore, nearly all the fat must be absorbed in solution as soaps.

We have not investigated the reaction of the intestinal contents in other animals during fat absorption, but in the rabbit, during carbohydrate absorption, it is strongly alkaline all the way, from pylorus to cæcum, and in the pig the mixed contents during the absorption of a mixed meal (meal and oats) had a strong alkaline reaction. As already stated, the filtered contents in these animals do not perfectly dissolve fat, and the portion dissolved must be in the form of soap, because the reaction remains alkaline to litmus after solution. In such animals it is probable that the greater part of the fat must be absorbed as soaps.

The main objections which have been urged against absorption of fats as soaps are, first, absorption in presence of an acid reaction in the dog, in which case it was supposed impossible that soaps could be present simultaneously in solution,† and, secondly, that the

* In this animal the intestinal contents are usually semi-solid. Care was taken to mix them so as not to obtain the alkaline surface reaction sometimes described. On thorough mixing an alkaline reaction was obtained.

† The acid reaction is also commonly supposed to preclude the possibility of the formation of an emulsion, and Cash ('Arch. f. Anat. u. Physiol.,' 1881, p. 386), in experiments chiefly made to determine this point, failed to find any emulsion within the dog's intestine. In ten out of sixteen experiments we obtained more or less emulsion, and in five of these, in almost the entire length, a perfect emulsion, containing immense numbers of minutest fat globules, and possessing a marked acid

amount of alkali required in the intestine for the absorption of all the fats of a fatty meal, as soaps, is out of all proportion to the amount actually present, being about twice the total alkalinity of the body.* The first objection has already been discussed; it has been shown that the acid reaction is due to weak organic acids, and that an alkaline reaction can be obtained by the use of a proper indicator, due to a compound of these weak acids with bases; in other words, to soaps.

The second objection may be met by the supposition that the same quantity of alkali acts cyclically as a carrier in conveying quantity after quantity of fatty radicle, as soap, from the intestine. The soaps are, it is known, broken up in the intestinal cells, and formed into fats by the action of the cell; in such a reaction alkali is set free, and there is no obvious reason why it should not be returned to the intestine and serve to carry a fresh portion of fatty radicle dissolved as soap into the epithelial cells. Such an action takes place in the acid secreting cell of the gastric gland, where sodium chloride is taken up from the blood, split into acid and alkali, and the alkali returned to the blood while the acid passes into the gland lumen; it is not, therefore, unreasonable to suppose that a similar action can take place in the intestinal absorbing cell.

We conclude that in certain animals, such as the dog, fats are absorbed partially as dissolved fatty acids, and partially as dissolved soaps; while in other animals, such as the white rat, fats are chiefly, if not entirely, absorbed as dissolved soaps.

“The Gaseous Constituents of certain Mineral Substances and Natural Waters.” By WILLIAM RAMSAY, F.R.S., and MORRIS W. TRAVERS, B.Sc. Received December 30, 1896, —Read February 4, 1897.

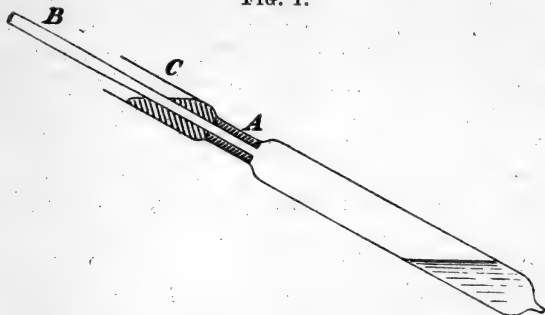
It is still uncertain whether helium is a single elementary gas or a mixture of two or more gases. If a mixture, it is probable that they should occur independently, and that the proportion of the constituent gases should vary in samples from different sources. During the past year the gases obtained from a large number of minerals and natural waters have been examined with a view to investigate this point, and, also, to determine whether any new gaseous element could be discovered. In every instance the results have been negative; no

reaction to litmus. Although fats are not absorbed in the form of an emulsion, it is evident that the formation of an emulsion in the intestine must enormously increase the surface exposed to the action of the intestinal fluids, and proportionately increase the rate at which the fats are decomposed and dissolved.

* Munk, ‘Virchow’s Archiv,’ vol. 95, 1884, p. 408.

indication of the presence of any new element has been obtained, nor has any abnormality been observed in the spectrum of any of the gases examined.

FIG. 1.



Method of Examination of the Mineral Substance.

The mineral was ground to fine powder in an agate mortar, and then mixed with about twice its weight of acid potassium sulphate. This mixture was placed in a hard glass tube, which was connected with a Töpler pump, and, after exhaustion, heated to a red heat by means of a large Bunsen burner. The gases evolved were pumped off and collected over mercury in a tube containing a little caustic potash solution. In some instances, however, the mineral was heated alone; the same result was obtained, but the evolution of gas takes place rather more slowly. In order to diminish any chance of leakage of air into the apparatus, the hard glass tube was connected with the pump in the manner shown in fig. 1. The tube was drawn out to a neck at the point A. A piece of thick-walled rubber tube was fitted over the end of the tube B connected with the pump, and it was then forced tightly into the neck of the hard glass tube. By pouring a little mercury into the cup C the joint could be made absolutely air-tight.

Examination of Minerals and Rocks.

Several samples of fergusonite, monazite, and samarskite were first examined, and were found to give quantities of helium up to 1.5 c.c. per gram.

Columbite (a variety of tantalite), an isomorphous mixture of niobate and tantalate of iron and manganese, gave 1.3 c.c. of gas consisting chiefly of helium.

Pitchblende, containing zirconium, obtained by Dr. Hillebrand from Colorado, gave 0.36 c.c. of gas per gram, of which 0.3 c.c. was helium. Another sample gave 0.27 c.c. of helium per gram.

Malacone, ZrSO_4 , from Hitteroe in Norway, was the only mineral in which argon was found. Five grams of the mineral gave 12 c.c. of gas unabsorbed by caustic soda. After explosion with oxygen, and absorption of the residual oxygen with phosphorus, about 0.1 c.c. of gas remained. The residue was introduced into a tube with aluminium electrodes which was sealed off from the pump and attached to a coil giving a discharge sufficiently powerful to heat the electrodes to a red heat. The nitrogen was quickly absorbed by the red-hot electrodes, and, as soon as the banded spectrum had disappeared, the lines of helium and argon became visible. The green line of the helium spectrum was very strong, and the glow in the tube was distinctly green.

The argon present was in too large quantity to be attributed to accidental leakage of air into the apparatus; but, in order to confirm this exceptional result, and also to determine whether the green effect in the tube was due entirely to the low pressure of the helium, the experiment was repeated with a larger quantity of the mineral. With 10 grams of the mineral a quantity of gas was obtained, which, after removal of nitrogen, gave a yellow glow in the vacuum-tube; argon was again present, and its second spectrum could be brought out very strongly by means of a jar and a spark-gap in the secondary circuit. The experiment was repeated a third time with the same result. This proved conclusively that malacone contains both argon and helium.

Cinnabar.—Five grams gave 0.5 c.c. of gas, which consisted only of carbon monoxide.

Cryolite.—7.6 grams gave only a minute bubble of carbon monoxide.

Apatite.—Six grams gave 0.5 c.c. of a gas consisting wholly of hydrogen and carbon monoxide.

Baryta-celestine.—No gas was evolved; the pump remained at a phosphorescent vacuum.

Serpentine.—This specimen was from the Riffelhorn, and has been analysed by Miss Aston;* 5 grams gave 4 c.c. of gas which consisted wholly of hydrogen.

Gneiss, from the Diamirai Glacier, directly below the peak of Nanga-Parbat, Kashmir, brought home by Dr. Collie: 3 grams gave 6 c.c. of hydrogen.

Scapolite, a silicate of calcium, magnesium, and aluminium, gave no gas.

Cobalt ore, containing a considerable quantity of manganese dioxide:—3.2 grams of mineral, heated alone, gave 35 c.c. of gas consisting wholly of oxygen.

* 'Geol. Soc. Journ.', 1896, p. 452.

Lava from Iceland :—Two specimens were examined ; in each case a little carbon dioxide was obtained.

Some specimens from the Kimberley diamond field, obtained from Mr. Crookes :—

Blue clay :—A considerable quantity of a mixture of hydrogen and carbon monoxide was obtained. After explosion with oxygen, no trace of gas remained.

Coarse-grained gravel and so-called “carbon” gave the same result.

Examination of Specimens of Meteoric Iron.

Specimens of meteoric iron were kindly sent for examination by Dr. Fletcher of the British Museum :—

Greenbrier County meteorite :—Ten grams of metal gave a fairly large quantity of gas on heating, which consisted wholly of hydrogen.

Toluca meteorite :—One gram gave 2·8 c.c. of pure hydrogen.

Charca meteorite :—One gram gave 0·28 c.c. of hydrogen.

Rancho de la Pila meteorite (‘Min. Mag.,’ ix, 153) :—One gram gave 0·57 c.c. of gas. It consisted of hydrogen.

Obernkirchen Meteorite, from Schaumberg-Lippe, Germany, described by Wicks and Wöhler (‘Pogg. Ann.,’ vol. 120, p. 509) :—One gram gave 2·6 c.c. of gas.

The gases from these meteorites were exploded with oxygen, and were found to contain no trace either of argon or helium, or of nitrogen. The carbon compounds present were possibly produced by the decomposition of the oil, &c., with which the shavings of meteoric iron had become contaminated.

It will be remembered that a previously examined specimen of meteorite was found to contain both argon and helium.

Examination of the Gases held in Solution by the Waters of certain Mineral Springs.

Old Sulphur Well, Harrogate.—One carboy of water gave 650 c.c. of gas from which, after circulation and sparking, 45 c.c. of argon were obtained. Spectroscopic examination of the gas proved that it contained nothing but argon.

Strathpeffer Wells.—One carboy of water gave 1 litre of a gas which, after sparking and circulation, gave 22 c.c. of pure argon. The gas was separated from these waters by the method described by Lord Rayleigh (‘Phil. Trans.,’ A, vol. 186, p. 220).

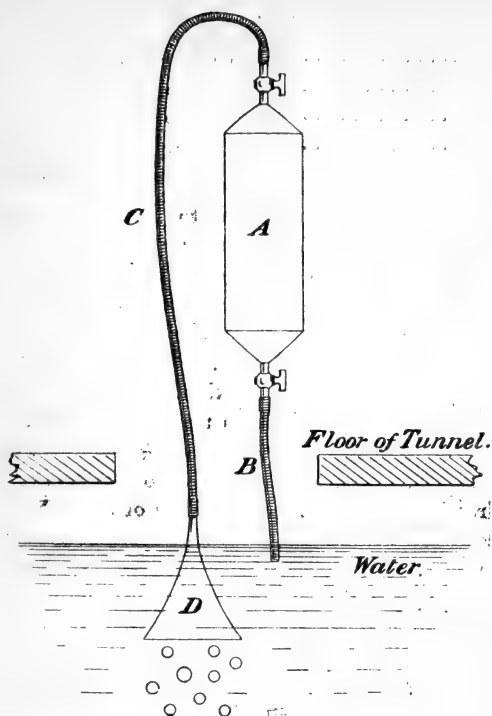
Mineral Springs of Caunterets.—The mineral springs of the Hautes Pyrénées, particularly those containing sulphides, have long been known to contain considerable quantities of nitrogen. Dr. H. C. Bouchard, of Paris, has recently (‘Compt. Rend.,’ vol. 121, p. 392) published an account of his examination of gases obtained from the wells

at Caunterets, which he has found to contain a considerable quantity of a mixture of argon and helium. He appears to have made a rough spectroscopic examination of the gases, and has stated in his paper that some of the lines in the red end of the spectrum do not belong to the spectrum either of argon or of helium. The author, a medical man, has dealt with the matter from a purely clinical standpoint, and his paper contains no data with regard to the supposed new lines.

To obtain samples of these gases, it was necessary to make a journey to Caunterets, and to visit the wells personally. Taking advantage of the Easter holidays, we left England provided with twelve tin cylinders, each with a capacity of 2 litres, for the purpose of collecting samples of gas from as many of the wells as we could obtain admission to. The management of the baths and wells granted us permission to visit the actual sources from which the baths, &c., are supplied, and courteously gave us every assistance, placing at our disposal the services of men connected with the different establishments. We were able to obtain samples of gas from four of the springs close to the town, but, on account of the deep snow, some of the more distant "sources" were quite inaccessible. The "sources" are for the most part situated at the end of tunnels driven for some distance into the hill-side. The water rises from below into tanks beneath the floor of the tunnels, and is conducted through pipes to the baths. Circular holes, about 9 inches diameter, in the floor formed the only means of inspecting the interior of the tanks. The gas appeared to rise with the water from natural springs in the bottom of the tanks; it was this gas that we collected for our investigation. The apparatus employed is shown in the accompanying figure. A piece of rubber tube B is fitted on to the lower tap of the cylinder A, which was then sucked full of water. The taps were then closed, and the cylinder fixed in a vertical position, the rubber tube hanging down into the tank. A second piece of rubber tube, C, was fitted on to the funnel D, which was lowered into the tank. Water was then drawn up into the rubber tube, which was immediately slipped over the nozzle of the upper tap on the tin cylinder. The taps were then opened, and the funnel brought over some point on the floor of the tank, from which gas was escaping. The gas rising into the funnel rapidly replaced the water in the cylinder which escaped back into the tank by the lower tube. In some of the wells a large quantity of gas could be collected in a short time, but in others the bubbles rose only very slowly.

Name of "source."	Temp.	Time required to fill vessels.
Raillère.....	39.5° C.	One tin in two hours.
Des Œufs.....	51.0 „	Three tins in 30 minutes.
Cæsar.....	46.0 „	One tin in four hours.
Espagnol.....	46.0 „	Three tins in about 15 minutes.

FIG. 2.



We proceeded with the examination of the gases immediately on our return to London. The gases were transferred to a glass gas-holder containing potash solution, and circulated over red-hot magnesium and copper oxide. The residual gas was pumped out of the circulating apparatus, and sparked with oxygen over potash to remove final traces of nitrogen.

Preliminary Spectroscopic Examination of the Gases.

Raillère.—Argon and helium, helium strong.

Des Œufs.—Argon, with less helium.

Espagnol.—Argon, with helium; the yellow and green helium lines very distinct, with jar and spark-gap.

Cæsar.—Argon, with a little helium.

The tubes were carefully compared with normal argon and helium tubes, but no new lines could be detected.

An attempt was made to separate the gas into its constituents by taking advantage of their relative solubilities. A measured quantity

of the gas was confined over a large quantity of boiled water, and the residue taken for examination.

Raillère	3.7 c.c. taken,	1.0 c.c. residue.
Des Œufs.....	8.5 ,,	4.0 ,,
Cæsar	2.2 ,,	0.5 ,,
Espagnol	8.0 ,,	(not measured).

The residue showed the helium lines rather more strongly.

The Des Œufs gas was submitted to fractional diffusion by the method described in the following paper.

The gas was divided into two portions by diffusion through a porous plug. These two fractions were then diffused separately, the light fraction of the heavy gas, and the heavy fraction of the light gas forming an intermediate fraction. This was again separated by diffusion into a heavy and a light portion, which were mixed with the heavy and light fraction obtained in the second stage. The process was repeated four times, and the resulting fractions, after sparking with a little oxygen, were rediffused so as to obtain the lightest sixth of the light fraction, and the heaviest sixth of the heavy fraction.

In a Plücker tube, the helium line, D₃, appeared somewhat stronger in the light gas, but the difference was not so marked as might have been expected. Neither of the tubes showed any lines other than those of the argon or helium spectrum.

The other samples of gas were not submitted to the diffusion process, as it did not seem probable that any results of value would be obtained.

In another paper it is shown that separation of helium from argon can be effected by taking advantage of the absorption of that gas by the platinum splashed on to the walls of the tube during the passage of the discharge. The gas is made to circulate at about 3 mm. pressure through a vacuum-tube with platinum electrodes, and kept cool by a water-jacket. The helium, together with any nitrogen or carbon compounds that may be present, is absorbed by the platinum, and may be liberated by heating the tube with a Bunsen's burner. The heavier fraction of the Des Œufs gas, and some of the gas from the Raillère were treated by this process, and the gas liberated from the platinum on heating was in each case introduced into a vacuum-tube with aluminium electrodes. The tube showed a banded spectrum which disappeared as the nitrogen was absorbed by the heated aluminium, leaving only normal helium at low pressure and a trace of argon. If any other gas, other than argon and helium, be present in the residue from the gas evolved from these various springs, after removal of the nitrogen, the methods employed have totally failed to bring it to light so far. It certainly cannot be present in any measurable quantity.

“Some Experiments on Helium.” By MORRIS W. TRAVERS,
B.Sc. Communicated by Professor W. RAMSAY, F.R.S.
Received December 30, 1896,—Read February 4, 1897.

In July of last year Professors Runge and Paschen (*Phil Mag.*, 1895, [ii], vol. 40, pp. 297—302) announced their discovery that the spectrum of the gas from clèveite indicated the presence of two elements. They also stated that by means of a single diffusion through an asbestos plug, they had been able to effect a partial separation of the lighter constituent, which was characterised by the green glow which it gave under the influence of the electric discharge in a vacuum-tube, and which was represented in the spectrum by the series containing the green line, $\lambda = 5015\cdot6$. Subsequently, at the meeting of the British Association at Ipswich, Professor Runge exhibited a tube containing the so-called green constituent; the colour of the glow differed strongly from that of an ordinary helium tube, but the gas contained in it was evidently at very low pressure, as phosphorescence was just commencing. Professor Runge has since acknowledged that the green effect in the helium tube may be produced by a change of pressure alone (*Astrophysical Journal*, January, 1896).

During an exhibition of the spectrum of helium at the soirée of the Royal Society on May 9, 1895, it was noticed that one of the Plücker tubes which had been running for nearly three hours, had become strongly phosphorescent. The tube was fitted with platinum electrodes, and the helium had apparently been absorbed by the platinum sparked on to the walls of the tube. We observed the same phenomena to take place on several subsequent occasions, but only in the case of tubes with platinum electrodes.*

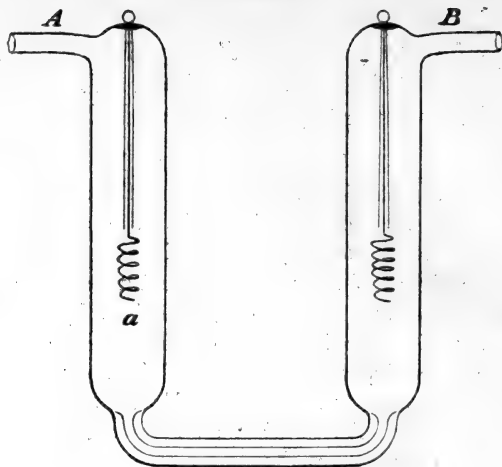
Now, if helium is not a single gas, it must consist of a mixture of two or more monatomic gases, capable of mechanical separation, and it is possible that one of its constituents might be absorbed by the platinum faster than the other. At the end of September, 1895, I commenced some experimental work on this subject, with the view of separating the two or more possible constituents from one another. The results were negative.

I employed in these experiments a piece of apparatus figured below (fig. 1).

A large Plücker tube, bent into a U-shape, has two side-tubes, A and B. The electrodes are of platinum, and project far into the tube; the straight parts, which are of thick wire, and about 30 mm.

* So far as I know, this phenomenon was first recorded by Professor Norman Lockyer (*Roy. Soc. Proc.*, 1895, vol. 58, p. 193).

FIG. 1.



long, are protected by a sheath of thin glass tube, the spirals at their ends being of thin platinum wire. The side-tube A is connected, by means of a tube containing pentoxide of phosphorus, with an apparatus for the introduction of gases into vacuum-tubes ('Trans. Chem. Soc.,' 1895, p. 686). The tube B is connected with a tap on the Töpler's pump. The apparatus was first thoroughly exhausted and heated by a Bunsen's flame, and then, after closing the tap on B, helium was introduced at about 3 mm. pressure. The electrodes were connected with the secondary terminal of a coil, and the current was turned on, making *a* the cathode. A deposit of platinum quickly appeared on the walls of the tube round *a*, and the following changes took place in the colour of the glow:—

1. Yellow, with slight tinge of red.
2. Bright yellow.
3. Yellowish-green.
4. Green; green line very strong.
5. Green, with phosphorescence.
6. Phosphorescent vacuum; spark passed between electrodes outside the tube.

The tube was then connected with the pump by opening the tap on B, but, as might have been expected, no trace of gas could be removed. The tap was again closed, and the tube was warmed carefully with a Bunsen's burner. The gas was slowly given off from the platinum, and on passing the discharge, colour-changes were observed to take place in the glow, from green to yellow.

From this experiment, it was obvious that the whole of the helium would be absorbed by the platinum splashed off, but it yet remained

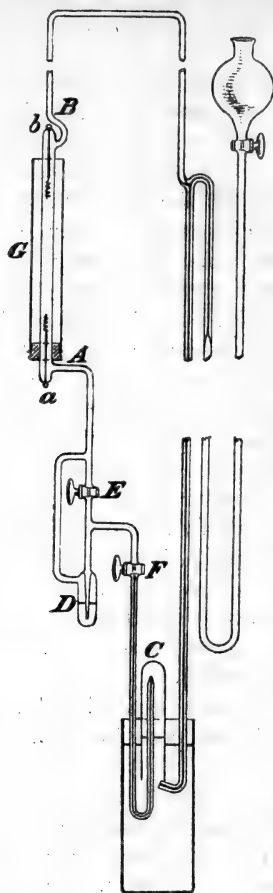
to be proved that the change in colour in the glow was not due to the absorption of the yellow constituent more quickly than the green one.

The vacuum-tube used in the last experiment was again filled with helium to about 3 mm. pressure, and the discharge was passed till the glow had become green, and the green line had reached its maximum intensity. Now, if any separation had taken place, the gas which had been absorbed by the platinum should contain a large proportion of the yellow constituent of helium, and should give a yellow glow in a vacuum-tube, even at low pressure. The remaining gas in the tube was, therefore, removed by pumping, and after closing the tap on B, the gas was driven off from the platinum, by warming with a Bunsen's flame. The current was then turned on, and a glow appeared of the green colour invariably shown by helium at low pressure. The change of colour in the tube during absorption of the helium is, therefore, to be entirely attributed to the lowering of the pressure. In describing these experiments I have used the term absorption in its general sense, as it is impossible to say at present whether we are dealing with a case of simple occlusion or not. The platinum, when it is deposited, is black and non-metallic in appearance, but, on heating, it assumes the colour and general character of ordinary platinum, and sometimes breaks away from the tube in thin scales. The change is probably the same as that which takes place when platinum-black is heated.

In a few of my experiments, I used helium containing traces of hydrogen, nitrogen, and carbon compounds. In these cases I found that not only was the helium absorbed, but also the other gases, to a greater or less extent. Hydrogen is readily absorbed, and next in order come carbon compounds and nitrogen. Argon is taken up only in very small quantity; in fact, this process serves as a method of separation of helium from argon, even when the helium is present to the amount of only 2 per cent.

To carry out this separation, the gas is made to circulate at about 3 mm. pressure, through a vacuum-tube of the type used in the last experiment. To effect this, the Töpler's pump is replaced by a Sprengel's pump, arranged as shown in fig 2, to deliver the gas removed from the vacuum-tube back into the tube C. To regulate the supply of gas entering the apparatus, the tap F was carefully turned, till the gas bubbled slowly through the mercury contained in the small tube D. The tap E served as a by-pass during the preliminary pumping-out of the apparatus, and was closed during the experiment. By carefully regulating the quantity of gas which entered the apparatus, and the rate of flow of mercury in the Sprengel's pump, it was possible to maintain a constant pressure in the apparatus for a long time.

FIG. 2.



To facilitate the absorption of the gases during the experiment, the vacuum-tube was kept cool by a water-jacket, *G*, closed at the bottom by a cork fitting tightly round the tube. When it was necessary to heat the vacuum-tube, the jacket could be loosened from the cork, and slipped up the side-tube *B*, which was bent round, and extended vertically for about 10 inches in a straight line with the vacuum-tube.

The gas was made to circulate for about six hours, and at the end of that time the tap *F* was closed, the tap *E* was opened, and the apparatus thoroughly exhausted. The jacket *G* was then raised, and the gas expelled from the platinum by heat was pumped off. From mixtures containing very little helium, a small quantity of that gas was separated, mixed with a trace of argon.

Kayser and Friedländer ('Chem. Zeitung,' vol. 9, p. 1529) have stated that in a vacuum-tube fitted with platinum electrodes, and containing atmospheric argon, the argon became absorbed by the deposited platinum, and the tube then showed certain of the helium lines. I have never been able to absorb argon to more than the very slightest extent, and though I have often had argon-tubes, which have become black, owing to the deposition of platinum, through which a powerful discharge has passed for many hours, I have never noticed any marked absorption.

A specimen of argon, the lightest fraction obtained from Professor Ramsay's diffusion experiments, was treated in the manner just described. After several hours' circulation it was found that the gas absorbed by the platinum consisted only of argon, and no trace of helium could be detected. This process has also been applied to the analysis of the gases from certain mineral springs; the results of these experiments form the subject of another paper.

"On the Gases enclosed in Crystalline Rocks and Minerals."

By W. A. TILDEN, D.Sc., F.R.S. Received December 19, 1896,—Read February 4, 1897.

It has long been known* that many crystallised minerals contain gas enclosed in cavities in which drops of liquid are also frequently visible. The liquid often consists of water and aqueous solutions, occasionally of hydrocarbons, and not unfrequently of carbon dioxide, the latter being recognisable by the peculiarities of its behaviour under the application of heat. The liquid supposed to be carbon dioxide has been found in some cases to pass from the liquid to the gaseous state, and therefore to disappear, and to return from gas to liquid at temperatures lower by two or three degrees than the critical point of carbon dioxide. This seems to indicate the presence of some incondensable gas, and as H. Davy found nitrogen in the fluid cavities of quartz, it seemed probable that the alteration of the critical point was due to that gas.

My attention was drawn to this subject by the observation that Peterhead granite, when heated in a vacuum, gives off several times its volume of gas, consisting, to the extent of three-fourths of its volume, of hydrogen ('Roy. Soc. Proc.,' vol. 59, p. 218).

* The chief literature of this subject is contained in the following papers:—Brewster, 'R. S. Edin. Trans.,' 1824, vol. 10, p. 1; 'Edin. J. Science,' vol. 6, p. 115; Simmler, 'Pogg. Ann.,' vol. 105, p. 460; Sorby and Butler, 'Roy. Soc. Proc.,' vol. 17, p. 291; Vogelsang and Geissler, 'Pogg. Ann.,' vol. 137, pp. 56 and 257; Hartley, 'C. S. Trans.,' 1876, vol. 1, p. 137, and vol. 2, p. 237, also 1877, vol. 1, p. 241.

Since this observation, I find that the presence of hydrogen in crystalline rocks has been recognised by other observers, notably by A. W. Wright ('Amer. J. Sci.,' Ser. 3, vol. 12, p. 171). In the course of a study of the gases from meteorites, Wright obtained from a certain "trap" rock, the origin and character of which is not stated, at a low red-heat, "about three-fourths of its volume of mixed gases, which were found to contain about 13 per cent. of carbon dioxide, the residue being chiefly hydrogen. Another specimen of trap containing small nodules of aporthite was examined at the request of Mr. G. W. Hawes, who had observed gas cavities in a thin section of the mineral prepared for microscopic examination. This gave off somewhat more than its own volume of gas, which was found to contain some 24 per cent. of carbon dioxide."

Professor Dewar and Mr. Ansdell have also examined one or two rocks in the course of their researches on meteorites ('Roy. Inst. Proc.,' 1886). They found that both gneiss and felspar, containing graphite, yield gas, which, upon analysis, was found to have the composition stated below.

	Occluded gas in volumes of the rock.	CO ₂ .	CO.	H ₂ .	CH ₄ .	N ₂ .
Gneiss.....	5.32	82.38	2.38	13.61	0.47	1.20
Felspar	1.27	94.72	0.81	2.21	0.61	1.40

Dewar and Ansdell remark that "the small quantity of marsh gas, no doubt, comes from the disseminated graphite, but the presence of the hydrogen is more difficult to explain, and requires further investigation."

I have lately been following up this question, and have obtained results which present some points of considerable interest. For materials I have been indebted chiefly to my colleague, Professor Judd, who has also supplied information as to the probable geological age of the specimens of rocks and minerals tested. All that I have examined yield permanent gas when heated in a vacuum. This gas varies in amount from a volume about equal to that of the rock or mineral to about eighteen times that volume. It usually consists of hydrogen in much larger proportion than that found by the observers just quoted, together with carbon dioxide and smaller quantities of carbon monoxide and hydrocarbons. Every specimen has been examined by the spectroscope for helium, but in no case could D₃ be recognised, or any other line which would lead to a suspicion of the presence of this substance. The gas is very frequently, but not always, accompanied by water in notable quantities.

The gas is apparently wholly enclosed in cavities which are visible in thin sections of the rock when viewed under the microscope, but as they are extremely minute, very little gas is lost when the rock is

reduced to coarse powder, and as a result of experiment in one or two cases, I find that practically the same amount of gas is evolved on heating the rock whether it is used in small lumps or in powder. In the first series of experiments undertaken with the object of a rapid survey of the materials, the gases were not completely analysed. They were collected, measured, the carbon dioxide removed by potash, and the residue examined by the spectroscope. When ignited in the air it always burned with a pale flame resembling that of hydrogen.

The table (p. 456) shows the results of these experiments.

A selection of these was then subjected to more careful and exact analysis. For this purpose fresh masses of the rock were taken, and the gas extracted in the usual way. The following are the results :—

	CO ₂ .	CO.	CH ₄ .	N ₂ .	H ₂ .
Granite from Skye	23·60	6·45	3·02	5·13	61·68
Gabbro from Lizard	5·50	2·16	2·03	1·90	88·42
Pyroxene gneiss, Ceylon.....	77·72	8·06	0·56	1·16	12·49
Gneiss from Seringapatam....	31·62	5·36	0·51	0·56	61·93
Basalt from Antrim	32·08	20·08	10·00	1·61	36·15

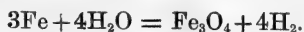
To account for the large proportion of hydrogen and carbonic oxide in these gases, it is only necessary to suppose that the rock enclosing them was crystallised in an atmosphere rich in carbon dioxide and steam which had been, or were at the same time, in contact with some easily oxidisable substance, at a moderately high temperature. Of the substances capable of so acting, carbon, a metal, or a protoxide of a metal, present themselves as the most probable.

The reduction of carbon dioxide or of water vapour by carbon gives rise to the formation of carbon monoxide, and if carbon had been the agent the proportion of this gas in the mixture must have been greater than is found to be the case. It is, of course, well known that carbon dioxide and water vapour are both dissociated at moderately high temperatures, but the greater part of the liberated oxygen recombines at lower temperatures, though a small portion may remain free in the presence of a large quantity of an indifferent gas or vapour. No free oxygen has been found in any of the gases analysed.

Direct experiments, made with ferrous oxide (obtained by gently heating pure chalybite) and with magnetic oxide of iron, show that while the former, at a red-heat, decomposes both steam and carbon dioxide quite freely, liberating hydrogen and carbon monoxide, and becoming itself oxidised into magnetic oxide; the latter has no action

Name of rock or mineral.	Locality.	Character.	Volume of gas per volume of rock.	Composition in 100 volumes.	
				CO ₂ .	H ₂ &c.
<i>Rocks of Tertiary Age.</i>					
Granite 1	Skye	Plutonic, acid	1.6	11.5	88.5
" 2	"	"	2.8	31.0	69.0
"	Salen, Mull.	"	1.3	34.7	65.3
Gabbro	L. Coruisk, Skye	" basic	3.5	21.6	78.4
Basalt	Antrim	"	8.0	32.0	68.0
<i>Rocks of Palaeozoic Age.</i>					
Quartzite	Durness (Sutherland)	Aqueous, altered	2.2	14.3	85.7
Gabbro	Lizard	Plutonic, basic	6.4	trace	100.0
Granite	Peterhead	" acid	2.6	24.8	75.2
"	Cornwall	"	4.3	8.8	91.2
<i>Rocks of Unknown Age (mostly Archaean).</i>					
Granite	Near Dublin	Plutonic, acid	5.0	9.4	90.6
"	Ardshiel	"	6.9	79.5	20.5
Greisen	Altenburg (Saxony)	" altered	1.8	13.6	86.4
Granulite	Central India	"	2.6	48.7	51.3
Quartz schist	Castlewellan (co. Down)	Metamorphic	2.8	23.0	77.0
Fuchsite schist	Baroda	"	4.2	20.8	79.2
Corundum rock	Pipra, S. Rewah, India	"	3.5	26.0	74.0
Pyroxene gneiss	Dombra (Ceylon)	"	7.3	84.4	15.6
Gneiss with corundum	Seringapatam	"	17.8	18.0	82.0
" " garnets and graphite	Doloswella (Ceylon)	"	4.5	11.0	89.0
"	Himalayas (Nanga Parbut)	"	7.2	11.5	88.5
<i>Recent Lava.</i>					
	Vesuvius, 1760	"	0.65	72.0	28.0
<i>Minerals.</i>					
Graphite	Ceylon	"	7.5	48.0	52.0
Quartz matrix of same	"	"	1.2	44.5	55.5
Beryl	Glencullen (co. Wicklow)	"	6.7	6.0	94.0
Tinstone	Straits Settlements	"	1.3	45.4	54.6

at all upon either steam or carbon dioxide. Magnetic oxide of iron is the final product of the action of steam or of carbon dioxide at a high temperature upon metallic iron:—



Now, metallic iron has been detected in basalts and some other rocks by Andrews ('Brit. Assoc. Rep.,' 1852, Sections, p. 34), and by other observers (*e.g.*, G. W. Hawes, 'Amer. J. Sci.,' Ser. 3, vol. 13, p. 33), and I have verified this observation in the case of the gabbro of Loch Coruisk. But it must be remembered that both the reactions indicated in the equations just given are reversible, and therefore the presence of metallic iron along with the magnetic oxide in such rocks cannot be taken by itself as final proof that the oxide and the associated gases, hydrogen and carbonic oxide, are the products of the action of steam and carbon dioxide upon metallic iron. The presence of marsh gas in these rocks and the production of large quantities of hydrocarbonous gases, as well as liquid petroleum, in many parts of the earth's surface, tend to support the view, which is apparently gaining ground, that in the interior of the earth's crust there are large masses, not only of metal but of compounds of metals, such as iron and manganese, with carbon. Assuming the existence of such material, it is easy to conceive how, by the action of water at an elevated temperature, it may give rise to metallic oxides and mixtures of hydrogen with paraffinoid and other hydrocarbons. This view was put forward some years ago by Mendelejeff ("Principles of Chemistry," Translation by Kamensky and Greenaway, vol. 1, 364—365), and it has lately received further support from the results of the study of metallic carbides, which we owe especially to Moissan ('Roy. Soc. Proc.,' vol. 60, 1896, pp. 156—160).

"On Lunar Periodicities in Earthquake Frequency." By C. G. KNOTT, D.Sc., Lecturer on Applied Mathematics, Edinburgh University (formerly Professor of Physics, Imperial University, Japan). Communicated by JOHN MILNE, F.R.S. Received November 4, 1896,—Read February 4, 1897.

(Abstract.)

1. *Introduction.*—The paper is a discussion of Professor Milne's Catalogues of 8331 earthquakes, recorded as having occurred in Japan, during the eight years 1885 to 1892 inclusive. These catalogues, forming vol. 4 of the 'Seismological Journal of Japan,'

are unquestionably the most complete ever constructed for an earthquake-disturbed country.

The discussion is really a working out of certain lines suggested in a paper on "Earthquake Frequency," communicated by me in May, 1885, to the Seismological Society of Japan, and published in vol. 9 of the 'Transactions' of that Society. In that paper I pointed out the importance of subjecting earthquake statistics to some strict form of mathematical analysis, and gave a simple arithmetical process for separating the annual and semi-annual periods in earthquake frequency. The results then obtained have been fully corroborated by Dr. C. Davison in his paper "On the Annual and Semi-annual Seismic Periods" ('Phil. Trans.,' vol. 184, 1893); and my suggestion that the annual period is connected with barometric pressure is also strongly supported by Dr. Ferd. Seidl in his pamphlet 'Die Beziehungen zwischen Erdbeben und Atmosphärischen Bewegungen' (Laibach, 1895). The semi-annual period, which was first clearly brought into evidence in my earlier paper, does not admit of a very ready explanation.

In my paper of 1885 I also considered in some detail the various tidal actions which might reasonably be supposed to have a determining influence on earthquake frequency. From lack of material it was not possible at that time to make a satisfactory search for lunar periodicities; but the remarkable fulness of information contained in Professor Milne's latest catalogues tempted me to undertake the labour involved in (first) tabulating the statistics in terms of lunar periods, and (second) analysing harmonically the tables so prepared.

2. *The Lunar Daily and Half-daily Periods.*—In one of the catalogues the earthquakes are classed according to district. Districts 1 to 6 lie on the N.E. and E. coasts of Japan, reckoning from the north; districts 6 to 11 on the S. coast; and 12 to 15 on the W. coast. Districts 6 and 7 are the most important, the former being the region including Tokyo and Yokohama, and the latter the region including Nagoya, which was the scene of the destructive earthquake of October 28, 1891. The investigation into a possible lunar daily period is conveniently based upon this classification into districts. Had that not been done by Professor Milne the labour involved in taking into account differences in local time would have been enormous; for, to compare the time of occurrence of a recorded earthquake with the immediately preceding meridian passage of the moon, it was necessary to apply corrections for longitude and local time.

The statistics for each district were, in the first instance, separated out and tabulated according to time of occurrence, estimated in hours after the immediately preceding passage of the moon. The method

is explained in full in the paper. To lighten in some measure the labour of the harmonic analysis, certain districts were thrown together to form a district group. Table I contains the number of earthquakes in each district or district group, which formed the material for discussion.

Table I.

District.	Number of earthquakes.	Description of district.
1	397	Nemura.
2—5	627	E. coast.
6	1432	S.E. corner.
7	3632	Nagoya, &c.
8	245	Kii Channel.
9—10	335	E. and S. of Kyushu.
11	384	W. of Kyushu.
12	112	W. coast of Main Island.
13	118	
14—15	145	

Of the tabulated numbers for each district or district group, overlapping means of every successive five were taken, and these were divided by the mean of all. The numbers so obtained represent relative frequencies throughout the lunar day, and are given in Table II, which also contains a like series for all the earthquakes taken in combination.

The most important are the frequencies for districts 6 and 7, and also for all combined. They are shown graphically in the figure (p. 461).

Each series of numbers was then discussed by harmonic analysis in accordance with the Fourier expansion

$$x = 1000 + \sum_{n=1}^{n=\infty} c_n \sin n \left(\frac{360t}{25} + \alpha_n \right),$$

where x is 1000 times the relative frequency at time t , estimated in hours after the meridian passage of the moon, and where the amplitude c_n and the phase α_n are to be calculated. The amplitudes and phases for the first four harmonics are given in Table IV.

There is a tendency for the second harmonic amplitude to be greater than the first, while in half the number it is the greatest of all. As regards the times of occurrence of the maxima for the different harmonics, there is no regularity except perhaps in the case of the second harmonic. In four (1, 6, 7, 8) the maximum of the second harmonic falls within two hours of the half time between the upper and lower meridian passage of the moon. In the others it falls within two hours of the times of upper and lower meridian passage.

Table II.—Relative Frequency of Earthquakes throughout the Lunar Day.

Hours.	Districts.										All Japan.
	1	2-5	6	7	8	9-10	11	12	13	14-15	
1	1.07	0.94	1.047	1.007	0.88	0.96	1.08	1.16	0.93	1.31	1.018
2	1.00	1.00	1.019	0.999	0.90	0.90	1.02	1.16	0.72	1.10	0.996
3	1.02	0.96	1.026	0.995	0.98	0.82	1.02	1.03	0.59	0.90	0.985
4	1.10	1.01	0.974	1.008	0.96	0.88	0.95	0.94	0.64	0.72	0.984
5	1.02	0.99	0.939	0.983	0.94	0.82	0.99	1.03	0.89	0.66	0.962
6	1.02	1.04	0.998	1.007	1.10	0.99	0.94	1.12	1.02	0.66	1.000
7	1.02	1.03	1.068	1.019	1.10	0.97	0.95	1.03	1.10	0.69	1.020
8	1.03	1.03	1.012	1.018	1.00	0.99	0.99	1.07	1.06	0.69	1.005
9	0.93	1.06	1.026	1.015	1.06	1.07	1.00	1.29	0.97	0.72	1.015
10	0.91	1.11	1.044	0.997	1.18	1.01	1.03	1.03	0.89	0.90	1.011
11	0.83	1.08	0.977	0.977	1.04	1.03	1.11	0.85	1.06	1.00	0.986
12	0.82	1.04	0.977	0.966	0.96	1.09	1.13	0.85	1.06	1.03	0.980
13	0.78	0.94	0.977	0.984	1.10	1.07	1.05	0.94	1.27	1.21	0.989
14	0.88	0.85	1.016	0.973	1.04	1.06	0.99	0.67	1.23	1.28	0.983
15	0.93	0.94	1.040	0.984	0.92	1.15	0.89	0.67	1.14	1.07	0.989
16	1.05	0.98	1.054	0.981	0.94	1.03	0.82	0.71	0.97	1.17	0.991
17	1.12	0.98	1.012	0.992	1.06	0.99	0.85	0.63	0.97	1.17	0.995
18	1.12	1.03	1.009	1.003	0.98	1.07	0.90	0.45	0.76	0.93	0.997
19	1.05	1.02	0.988	1.023	0.96	0.96	0.94	0.67	0.76	0.86	0.995
20	1.06	0.96	0.970	1.018	1.02	0.88	0.98	0.76	0.97	1.00	0.994
21	1.03	0.96	0.921	1.032	1.04	1.00	1.05	1.16	1.02	0.86	1.004
22	1.05	0.95	0.921	1.028	0.98	1.06	1.05	1.34	1.14	1.00	1.010
23	1.05	1.03	0.974	1.004	0.94	1.04	1.04	1.52	1.31	1.27	1.023
24	1.05	1.03	0.995	0.984	0.98	1.03	1.12	1.43	1.40	1.41	1.025
25	1.10	1.01	1.006	1.021	0.94	1.13	1.12	1.52	1.10	1.38	1.043

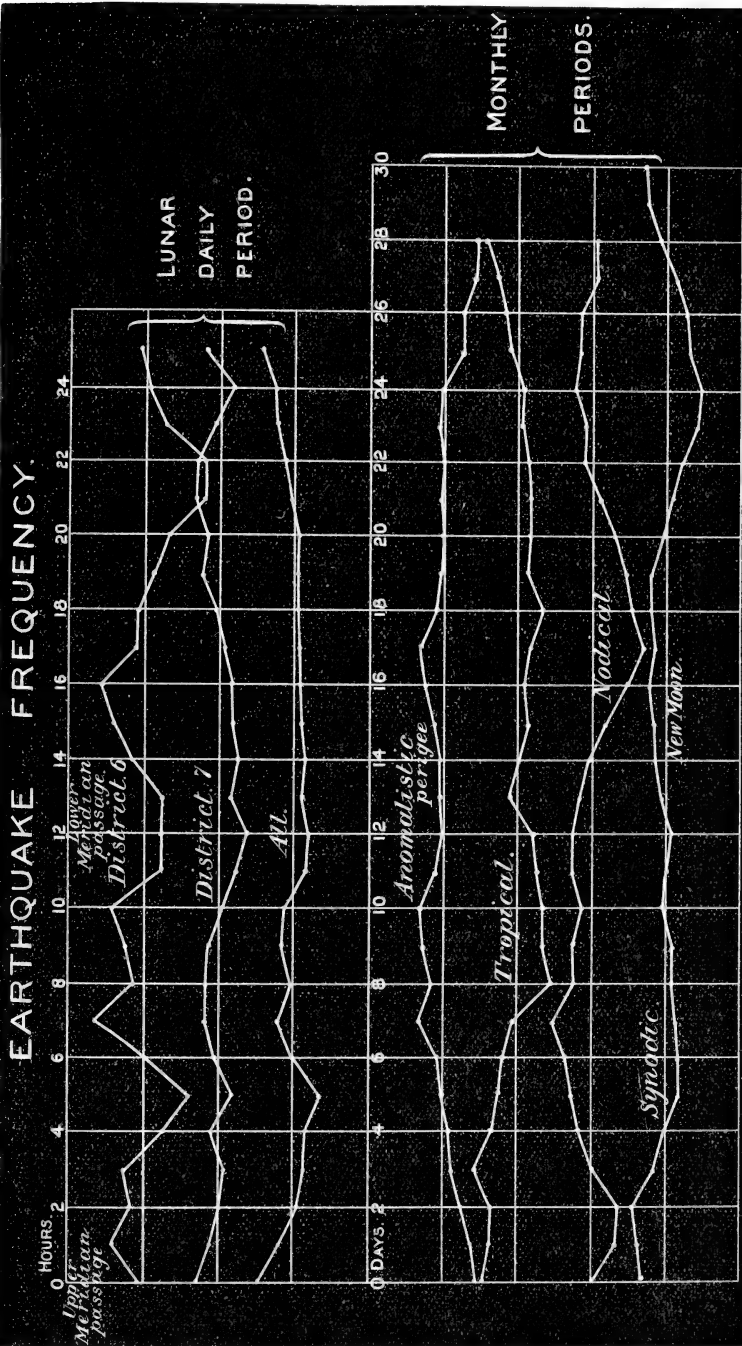


Table IV.—The Coefficients c and α , the amplitudes and phase-coefficients.

District.	c_1 .	c_2 .	c_3 .	c_4 .	α_1 .	α_2 .	α_3 .	α_4 .
1	94.4	68.7	46.9	16.6	7.85	9.95	2.08	3.43
2—5	29.9	36.5	32.4	35.2	24.2	4.33	0.88	2.95
6	18.4	20.7	29.9	14.6	21.8	11.5	1.79	5.65
7	13.0	16.4	3.17	8.98	6.7	7.9	1.3	6.1
8	54.2	45.8	10.0	6.17	20.2	9.03	2.53	2.07
9—10	65.0	56.9	53.8	5.26	15.9	3.94	3.0	1.92
11	42.8	91.5	32.5	26.0	3.55	4.19	6.91	1.53
12	245.0	233.0	111.5	36.7	3.33	4.93	3.57	4.48
13	73.9	167.0	193.0	7.48	15.1	4.62	3.9	1.09
14—15	175.0	247.6	91.9	41.5	11.2	2.78	2.79	1.75
All	10.3	17.9	10.9	3.97	6.62	7.97	2.42	2.43

A comparison of these times with the times of high water in the various districts failed to establish any relation. We are forced to the conclusion that if there be any lunar-diurnal periodicity imposed upon earthquake frequency, it is the result of tidal stresses acting directly on the approximately rigid crust of the earth, and not indirectly through the loading due to the ocean tides.

Because of the comparatively great number of earthquakes the results for districts 6 and 7 are the most important. During the eight years under discussion, the shocks in district 6 occurred with normal frequency. All were comparatively small; none were disastrous. On the other hand, the case of district 7 is altogether peculiar. In general, this is a comparatively quiet district; but the great disaster of October 28, 1891, was followed by a vast number of after-shocks. These show distinct daily and half-daily periodicities, the latter having the greater amplitude. Thus, from district 6, with its 1432 earthquakes distributed with fair uniformity over eight years of normal activity, and from district 7 with its 3632 earthquakes, almost wholly included in a short fierce interval of fourteen months, we obtain very similar evidence as to the existence of a lunar half-daily period in earthquake frequency.

The results for "All" depend, in the main, upon the statistics for districts 6 and 7. The curious way in which the comparatively prominent 1st harmonics of these two districts tend to cancel one another, is a warning of the danger of lumping together statistics of different countries or different seismic areas in the search for possible periodicities.

3. *The Lunar Monthly and Fortnightly Periodicities.*—There are five distinct kinds of months recognised by astronomers, namely:—

- (1) The anomalistic month (27·545 days).
- (2) The tropical month (27·322 days).
- (3) The synodic month (29·531 days).
- (4) The sidereal month (27·3228 days).
- (5) The nodical month (27·212 days).

Of these, the last two cannot be regarded as having any influence on earthquake frequency, for the only conceivable effect is a tidal one, and the sidereal and nodical months have no necessary tidal relations. At the same time the periods of the sidereal and tropical months are so nearly the same that they can hardly be discriminated in the lapse of eight years. On the other hand, the anomalistic month may show itself in earthquake frequency, since the moon in perigee has a greater tidal action than when it is in apogee. Again, because of the moon's variation in declination, being now north of the Equator, now south, we may reasonably search for a tropical monthly periodicity. And, finally, the synodic or common month may make itself apparent, there being possibly a greater tidal stress when the moon is in syzygy (as in ordinary spring tides) than when the moon is in quadrature (as in neap tides).

The earthquakes were accordingly tabulated according to these four months, whose periods differ appreciably; the nodical month being also included. For, by analysing the statistics in terms of both the tropical and nodical months, we may be the better able to draw conclusions as to the real existence of one or other periodicity. The relative *daily* frequencies, as finally reduced, are given in Table VI, and the curves are shown in the figure.

As in the case of Table II, each of the tabulated numbers is the mean of five successive numbers, and is regarded as belonging to the time of the middle one of these five.

It should be mentioned—and the remark applies also to the former cases—that the number of earthquakes which really occurred during the last time interval was increased in the proper ratio; so that the frequency during this last interval was made comparable with the frequencies of the other intervals. It was interesting to find how admirably the number so obtained harmonised with its neighbours of the first and penultimate interval.

In all cases the obvious aftershocks of any earthquake occurring on the same day were neglected. The 3000 aftershocks of the great disaster of October 28, 1891, were also left out.

The earthquakes on which the discussion is based numbered from 4725 to 4741, the number varying slightly for each monthly period, since, at the beginning and end of the eight years' interval, there were always a few, differing for the different months, which did not make up a complete period, and were, consequently, neglected.

Each series of numbers was analysed harmonically as far as the

Table VI.—“Monthly” Frequencies.

Day.	Anomalistic, from apogee.	Tropical, from 0° decl. N. to S.	Nodical, from ascending node.	Synodic, from full moon.
1	0·919	1·077	0·937	1·064
2	0·945	1·072	0·925	1·081
3	0·976	1·107	0·998	1·029
4	0·980	1·069	1·032	1·000
5	0·999	1·052	1·056	0·961
6	1·013	1·040	1·068	0·963
7	1·061	1·006	1·103	0·964
8	1·033	0·902	1·045	0·984
9	1·058	0·928	1·051	0·980
10	1·064	0·930	1·025	1·002
11	1·023	0·945	1·050	0·999
12	1·002	0·952	1·047	0·983
13	1·005	1·020	1·037	1·009
14	1·012	1·000	1·011	1·029
15	1·021	0·978	0·964	1·030
16	1·048	0·982	0·901	1·042
17	1·061	0·974	0·858	1·032
18	1·022	0·936	0·896	1·039
19	1·010	0·969	0·901	1·039
20	1·004	0·967	0·939	1·005
21	1·006	0·964	0·981	0·985
22	1·000	0·975	1·018	0·965
23	1·017	0·991	1·011	0·918
24	1·004	0·987	1·044	0·905
25	0·952	1·020	1·028	0·939
26	0·955	1·035	1·028	0·945
27	0·920	1·054	0·992	0·973
28	0·906	1·086	0·994	1·020
29	—	—	—	1·045
30	—	—	—	1·060

first four harmonics, according to a formula identical with that already given, due regard being paid to the different periods and the time unit involved. The results are given in Table VII, the phase coefficients being given in days.

Table VII.—Amplitudes and Phases.

“Month.”	c_1 .	c_2 .	c_3 .	c_4 .	α_1 .	α_2 .	α_3 .	α_4 .
Anomalistic ...	46·2	47·8	12·9	16·5	21·7	8·5	5·2	6·2
Tropical	54·7	40·7	23·1	17·2	6·0	1·9	7·9	2·4
Nodical	49·5	55·2	28·3	17·6	1·2	7·9	6·9	2·7
Synodic	11·0	52·1	24·5	4·7	13·7	2·7	7·7	0·6

A study of these tables discloses the presence of certain features which have no *raison d'être* on any rational theory of tidal stress.

The most important of these is the fact that the nodical month, which has no direct connexion with tidal stress periodicity, is characterised by harmonic amplitudes greater, on the average, than those corresponding to the other months. This is particularly evident in the graphs.

There are, however, other features which favour the hypothesis of seismic tidal stress, such as the occurrence in the vicinity of perigee of the Anomalistic 1st Harmonic amplitude; the lagging, by one day, behind full and new moon of the Synodic 2nd Harmonic maxima; the distinctly greater amplitude of the Synodic 2nd Harmonic as compared with those of the other harmonics—a fact which is in accord with the fortnightly succession of spring tides.

It is, certainly, a striking fact that the same statistics which, when grouped according to an approximately twenty-eight days' period, give a prominent 1st harmonic should, when grouped according to an approximately thirty days' period, give a comparatively small 1st harmonic but a prominent 2nd harmonic.

4. *General Conclusions.*—The conclusions are summarised under eight heads.

- (a) There is evidence that the earthquake frequency in Japan is subject to a periodicity associated with the lunar day.
- (b) The lunar half-daily period is particularly in evidence, both by reason of its relative prominence and the regularity with which, in each of two groups of the several seismic districts, its phase falls in relation to the time of meridian passage of the moon.
- (c) There is no certain evidence that the loading and unloading due to the flow and ebb of ocean tides have any effect on seismic frequency.
- (d) Hence we must look to the direct tidal stress of the moon, in its daily change, as the most probable cause of a range in frequency which does not exceed 6 per cent. of the average frequency.
- (e) There is distinct evidence, both as regards amplitude and phase, of a fortnightly periodicity associated with the times of conjunction and opposition of the sun and moon.
- (f) No definite conclusion can be drawn from the apparent monthly and fortnightly periodicities which seem to be associated with the periodic changes in the moon's distance and declination, for the simple reason that fully as prominent harmonic components exist when the statistics are analysed according to the periodic change in the moon's position relative to the *ecliptic*, and with this particular period no tidal stresses can be directly associated.

- (g) Nevertheless, the value of the phase lends some support to the view that there is a real connexion between the change in the moon's distance and earthquake frequency, since the maximum frequency falls near the time of perigee.
- (h) These conclusions have, in comparison with previous similar investigations, a peculiar value, inasmuch as they are based upon accurate statistics of fully 7000 earthquakes occurring within eight years in a limited part of the earth's crust, throughout which the seismic conditions may be assumed to be fairly similar from point to point.

February 11, 1897.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

Communications from Professor OLIVER LODGE, F.R.S., and Dr. LARMOR, F.R.S., on the recent discovery by Dr. P. Zeeman of the effect of a magnetic field on the light emitted by a soda flame, were read by the Secretary.

The following Papers were read :—

- I. "The Oviposition of *Nautilus macromphalus*." By ARTHUR WILLEY, D.Sc., Balfour Student of the University of Cambridge. Communicated by ALFRED NEWTON, M.A., F.R.S., on behalf of the Managers of the Balfour Fund.
- II. "Report to the Committee of the Royal Society appointed to investigate the Structure of a Coral Reef by boring." By W. J. SOLLAS, D.Sc., F.R.S., Professor of Geology in the University of Dublin.
- III. "The artificial Insemination of Mammals and subsequent possible Fertilisation of their Ova." By WALTER HEAPE, M.A., Trinity College, Cambridge. Communicated by FRANCIS GALTON, F.R.S.
- IV. "On the Regeneration of Nerves." By ROBERT KENNEDY, M.A., B.Sc., M.D. (Glasgow).

"The Oviposition of *Nautilus macromphalus*." By ARTHUR WILLEY, D.Sc., Balfour Student of the University of Cambridge. Communicated by ALFRED NEWTON, M.A., F.R.S., on behalf of the Managers of the Balfour Fund. Received February 3,—Read February 11, 1897.

Nautilus macromphalus is the species of nautilus characteristic of the New Caledonian Archipelago, which comprises the islands of New Caledonia, the Isle of Pines, and the Loyalty Group. I took up my residence on the shores of Sandal Bay, Lifu, in August, 1896. Having collected a number of *Nautilus*, I placed them in captivity in a large native fish-trap, specially fitted up, fed them twice or three times a week with fish, land-crabs, *Palinurus*, and *Scyllarus*, and on December 5, 1896, commenced to obtain the fertilised ova.

It is not necessary at present to describe the details of manipulation, and I therefore proceed at once to give a brief account of the more obvious features of the eggs as illustrated by the accompanying figures. The eggs are laid singly and at night, in concealed situations, and are firmly attached by a sponge-like reticulate area of attachment placed towards their hinder inflated extremity, usually on one face of the egg-case, but sometimes quite posteriorly, to a suitable surface. I supplied the latter to the *Nautilus* by fixing pieces of old sacking to the walls of the fish-basket, leaving loose, overhanging folds, beneath which the eggs could be well concealed. The fibres of the sacking were deftly employed by the *Nautilus* in cementing their eggs.

The ovum is enclosed within a double casing, an inner closed capsule, and an outer capsule more or less freely open in front. The material of which the capsules consist is of a bright milk-white colour, and of firm cartilaginous consistency. The capsules do not collapse, but retain their shape when allowed to dry.

For convenience of description, the exposed surface of the egg may be spoken of as the dorsal or upper side, while the attached side may be referred to as the lower or ventral side. The outer capsule is separate from the inner capsule below and for about two-thirds of the upper side, but is fused with it in the postero-dorsal region. Where the two capsules are fused together the covering of the ovum is much thickened.

The egg with outer covering complete is of remarkably large size, attaining a length of 45 mm., everything included, with a width of 16 mm., and a maximum height of 16.25 mm. The length and the width are fairly constant in normally shaped eggs, but the height varies somewhat, some eggs being a good deal flatter than others.

In fig. 1 an egg is represented as seen in its usual natural attached position. The depressed or "anterior" end of the egg is, as a rule, directed vertically upwards. The outer capsule is continued in front into two thin, translucent, terminal processes. For nearly half the length of the egg on the upper side the two halves of the outer capsule are separated by a narrow slit from one another and join together behind the centre of the egg. The dorsal ridge or suture of the inner capsule can be seen through this slit in the outer capsule. On the lower side of the egg the two halves of the outer capsule are continuous across the middle line throughout the length of the egg, except at the extreme anterior end.

The surface of the egg in the posterior inflated region is smooth, with a few slight folds like the folds of drapery, giving it a graceful

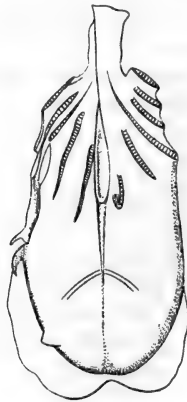


FIG. 1.—Fertilised egg of *Nautilus macromphalus* in the natural attached position. The pectinate ridges and fenestrations, together with the slit in the wall of the outer capsule, are well seen. The arcuate thickening in the middle of the posterior half of the egg is due to the fusion of the outer with the inner capsule. In this ovum the anterior membranous prolongations of the outer capsule were unequal, the larger of them having the form of a thin flattened expansion.



FIG. 2.—The same egg from the side, showing the inflated posterior or proximal portion and the more flattened distal portion, as also the spongy area of attachment.

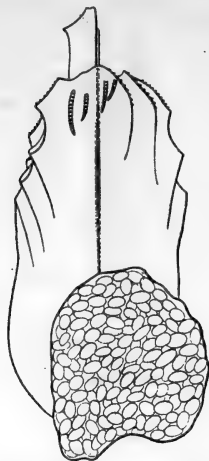


FIG. 3.—The same egg as in the preceding figures, from below. Behind is the somewhat irregularly shaped spongy area of attachment.

appearance. The anterior depressed region is characterised by the presence of a number of pectinate ridges and of fenestrations in the wall of the outer capsule (figs. 1—3). Sometimes, however, the pectinations are obscure and the fenestrations may be absent.

Hardly will any two eggs present an exactly similar appearance. Sometimes there are shred-like processes from the surface of the outer capsule, lending a more or less tattered appearance to the egg.

In fig. 4 another egg is shown with the above-described slit in the upper wall of the outer capsule, widened out so as to disclose the inner capsule to view.

The inner capsule has a regular oval shape with anterior pointed extremity and a generally smooth surface. Its wall has a finely striated structure, the striæ having a watery appearance. There are three distinct seams or sutures, representing lines of least resistance, in the wall of the inner capsule, namely, a median suture on the upper side (*i.e.*, the side directed away from the attached side of the egg), and two lateral sutures placed towards the lower surface of the capsule (figs. 4—6).

The dorsal suture is marked by a prominent ridge which is produced in front beyond the anterior extremity of the main body of the inner capsule into a slender terminal appendix.

The lateral sutures are marked by less prominent ridges, and are continued into one another anteriorly, immediately behind the anterior extremity of the inner capsule. In consequence of the continuity of the lateral sutures, the lower side of the egg can be raised up like a cap or an operculum. The inner capsule is often easily



FIG. 4.—Another egg of *N. macromphalus*, seen from above, with the longitudinal slit in the upper wall of the outer capsule widened out so as to expose the inner capsule to view.

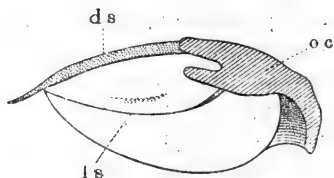


FIG. 5.—Inner capsule of another egg to show the dorsal ridge along the dorsal suture (*d. s.*) with its anterior terminal prolongation, and the lateral suture (*l. s.*). *o. c.*, remains of outer capsule.

ruptured along the sutures. In the middle line of the lower surface of the inner capsule there is a slight longitudinal groove, and other unimportant grooves often occur. Where the outer capsule is united to the inner capsule there is usually a depression or flattening in the wall of the latter.

The vitellus (fig. 6) does not fill the entire cavity of the inner capsule, but is surmounted by a layer of colourless, somewhat cloudy, viscid albumen which is massed up, as it were, at the two extremities of the egg. The yolk is of a rich brown colour, of very fluid consistency, and sub-translucent. The surface of the vitellus is quite smooth. The length of the inner capsule is about 26 mm., while that of the enclosed vitellus is 17 mm.

I am not in a position to say much about the embryonic area at present, but I have observed an area pellucida about the middle of

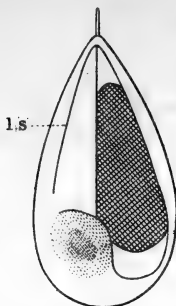


FIG. 6.—The inner capsule of the same egg, seen from below (*i.e.*, from the side directed towards the surface of attachment). Half the lower wall of the capsule has been removed by slitting along one of the lateral sutures, and along the median groove (mentioned in the text), to show the brown-coloured vitellus lying in the capsule. The continuity of the lateral sutures (*l. s.*) in front is well seen. The shaded area represents a depression which occurred in the wall of the inner capsule in the region of the area of attachment of the outer capsule.

the lower surface of the vitellus in an egg which had been allowed to develop for twenty-four hours after being first seen. The large quantity of yolk points to the occurrence of a long period of incubation.

Sometimes the capsules of the egg are malformed, and, on opening such an egg, the vitellus is found to be already ruptured.

From the fact that in New Britain I obtained mature males of *Nautilus pompilius*, carrying a spermatophore in the cephalic region throughout the year, I came to the conclusion that the reproduction of *Nautilus* took place all the year round. It now seems probable that the breeding of *Nautilus*, as of so many other forms, is subject to a definite law of periodicity.

Finally, it may be mentioned that *N. macromphalus* varies with regard to the position of the spadix on the right or left side, and also as to the origin of the siphuncular artery, in the same way as *N. pompilius* does. The male of *N. macromphalus* carries a spermatophore in the same position as in *N. pompilius*; and, in fact, the only essential difference between the two species that I know of at present, is the difference between the shells in the umbilical region.

“On the Regeneration of Nerves.” By ROBERT KENNEDY, M.A., B.Sc., M.D., Glasgow. Communicated by Professor McKENDRICK, F.R.S. Received January 7,—Read February 11, 1897.

(Abstract.)

The author treats the subject under the following heads:—

- I. A short historical and critical review of the books and papers which have appeared on the subject from the time of Cruikshank (1776).
- II. Clinical reports of four cases of secondary suture of nerves as follows:—

1. Suture of the median and ulnar nerves six and a half months after division in the middle of the forearm. There was total loss of sensation and motion in the distribution in the hand, and marked atrophic changes. Three days after the operation, sensation commenced to return; by the nineteenth day, touch was correctly localised on all parts of the fingers; and by the end of the first month, sensation was almost perfect. Improvement in motion was slow and imperfect.

2. Suture of the median three months after complete division above the wrist. Sensation was lost in the median distribution, and opposition of the thumb was impossible. There was marked atrophy of the thenar eminence. Two days after the operation, sensation commenced to return. Both sensation and motion speedily improved, and by the end of a year recovery was almost perfect.

3. A case in which the median, musculo-spiral, and ulnar were involved in cicatricial tissue at the seat of fracture at the elbow joint; excision of portions from median and musculo-spiral, and suture, two months after accident. There was total anæsthesia in the distribution of the affected nerves, and paralysis of the muscles. Sensation, after the operation, commenced to return on the fourth morning, but made slow progress. The case was under observation for six weeks only, at which time no improvement had occurred in motion, but sensation was present in the fingers.

4. Suture of the ulnar nerve eighteen months after division. Sense of pain was totally lost in the ulnar distribution. Five days after the operation, sense of pain returned in the little finger, and by six weeks, sensation was almost perfect, although motion had not improved.

III. Deductions from the results of operation.

From the above results the author concludes that the early return

of sensation must be regarded as indicating a restored conductivity of the divided nerve. He holds that the theories which have hitherto been advanced to account for early return of sensation apart from reunion of the nerve, are inapplicable to cases where *early* return of sensation occurs from suture, performed after the lapse of *several months* from the time of section. The imperfect return of motion he takes to be fully explained by the fact that the muscles have undergone great trophic change, or indeed total destruction, and that, therefore, their restitution must be slow, or may even be impossible.

IV. Microscopical examination of the portions removed previous to suture.

Both the central and peripheral ends of nerves which had not reunited in any way, contained young nerve fibres grouped in bundles, each bundle containing, as a rule, many fibres. The fibres contained an axis-cylinder lying in the centre of a clear, well-defined zone, which, again, contained a granular, myeline deposit, while spindle-shaped nuclei were attached to the sides of the fibres at frequent intervals. Where the ends of the nerve were united by a cicatricial segment without conductivity being restored, the examination of the segment showed a dense network of connective tissue containing in its meshes bundles of young fibres.

The portions excised from the nerves involved at the seat of fracture showed at their central ends a normal structure, but elsewhere no trace of old myeline fibres, nor of degenerated fibres; but the section was made up of young fibres in bundles, which bundles were of only slightly greater diameter than the old myeline fibres, and often surrounded by a delicate sheath. At the point of transition from old to young fibres, many of the old myeline fibres contained an enlarged nucleus, with one or two distinct young fibres lodged between the sheath of Schwann and the myeline sheath. In other cases the number of young fibres lying in a similar position was greater. All stages up to complete replacement of the old myeline sheath and axis-cylinder by young fibres were found.

V. Deductions from the microscopical examination.

1. Degeneration :—

(a) That there is no evidence of ascending degeneration of the kind described by Krause after interruption of a nerve.

(b) That the old axis-cylinder and myeline sheath are destroyed in the peripheral segment, and in the ultimate portion of the central segment.

2. Regeneration :—

(a) That young nerve fibres are developed in the peripheral segment, as well as in the end of the central segment, and that even while there is no connexion between the two ends.

(b) That these young nerve fibres originate within the old sheath of Schwann from the protoplasm and nucleus of the interannular segment. The spindle-cells formed from the protoplasm and nuclei of the interannular segments elongate and unite to form protoplasmic threads, with the elongated nuclei attached to their sides. The central portion of the protoplasmic thread develops into the axis cylinder, while myeline is deposited in drops in the protoplasm surrounding the newly formed axis-cylinder. The protoplasm in which the myeline is deposited remains with the nucleus as the neuroblast of the new interannular segment.

(c) That so long as conductivity of the nerve is not re-established, the development of the fibres proceeds only to a certain stage, and as the new fibres three months and eighteen months subsequent to division present identical characters, this stage may be regarded as a resting stage, depending for its further development on re-establishment of function.

(d) That cicatricial intercalary segments reuniting the ends of a divided nerve may be permeated by young fibres from end to end without re-establishment of function, if the amount of cicatricial connective tissue present in the mass is sufficient by its pressure to prevent the passage of impulses.

February 18, 1897.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

- I. "On the Iron Lines present in the Hottest Stars. Preliminary Note." By J. NORMAN LOCKYER, C.B., F.R.S.
- II. "On the Significance of Bravais' Formulæ for Regression, &c., in the case of Skew Correlation." By G. UDNY YULE. Communicated by Professor KARL PEARSON, F.R.S.
- III. "Mathematical Contributions to the Theory of Evolution. On a Form of Spurious Correlation which may arise when Indices are used in the Measurement of Organs." By KARL PEARSON, F.R.S., University College, London.
- IV. "Note to the Memoir by Professor Karl Pearson, F.R.S., on Spurious Correlation." By FRANCIS GALTON, F.R.S.

“On the Iron Lines present in the Hottest Stars. Preliminary Note.” By J. NORMAN LOCKYER, C.B., F.R.S. Received January 25,—Read February 18, 1897.

In continuation of investigations communicated to the Royal Society in 1879* and 1881,† on the effect of high-tension electricity on the line spectra of metals, I have recently used a more powerful current and larger jar surface than that I formerly employed.

The former work consisted in noting (1) the lines brightened in passing a spark in a flame charged with metallic vapours, and (2) the lines brightened on passing from the arc to the spark. It was found, in the case of iron, that two lines in the visible spectrum at 4924·1 and 5018·6, on Rowland's scale, were greatly enhanced in brightness, and were very important in solar phenomena.

The recent work carries these results into the photographic region. The result is interesting and important, since seven additional lines have been found to have their brightness enhanced at the highest temperature. These, as well as the two previously observed, are shown in the following table, which also indicates the behaviour of the lines under different conditions, as observed by Kayser and Runge (K and R) and myself (L) in the arc, and by Thalèn (T) and myself in sparks:—

Lines of Iron which are enhanced in Spark.

Wave-length.	Intensity in flame.	Intensity in arc (K and R). Max. = 10.	Length in arc (L). Max. = 10.	Intensity in spark (T). Max. = 10.	Intensity in hot spark (L). Max. = 10.
4233·3	—	1	—	—	4
4508·5	—	1	—	—	4
4515·5	—	1	—	—	4
4520·4	—	1	—	—	2
4522·8	—	1	3	—	4
4549·6	—	4	5	—	6
4584·0	—	2	4	—	7
4924·1	—	1	3	6	6
5018·6	—	4	—	—	6

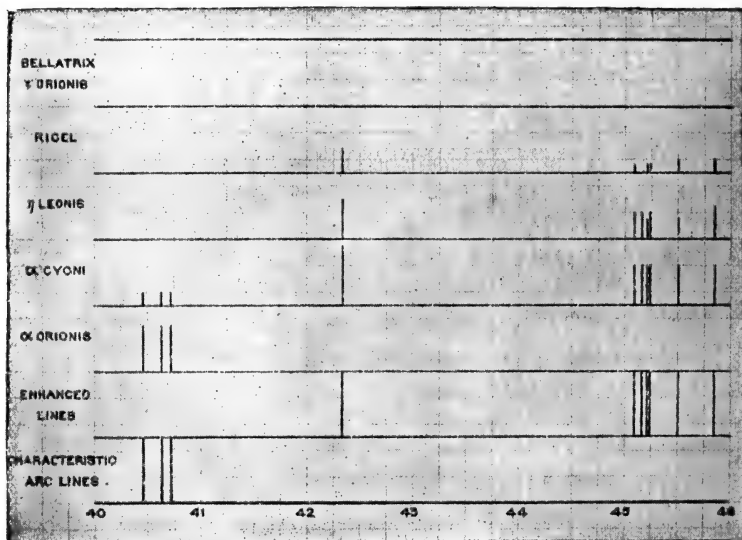
Combining this with former results, we seem justified in concluding that, in a space heated to the temperature of the hottest spark, and shielded from a lower temperature, these lines would constitute the spectrum of iron.

* ‘Roy. Soc. Proc.’ 1879, vol. 30, p. 22.

† *Ibid.*, 1881, vol. 32, p. 204.

Defining the hottest stars as those in which the ultra-violet spectrum is most extended, it is known that absorption is indicated by few lines only. In these stars iron is practically represented by the enhanced lines alone; those which build up, for the most part, the arc spectrum are almost or entirely absent.

The intensities of the enhanced lines in some of the hottest stars are shown in the appended diagram, and, for the sake of comparison, the behaviour of a group of three lines which are among the most marked at lower temperatures, is also indicated. In addition, the diagram shows the inversion in intensities of the spark and arc lines in the spectrum of a relatively cool star—such as α -Orionis.



The facts illustrated by the diagram indicate that the enhanced lines may be absent from the spectrum of a star, either on account of too low or too high a temperature. In the case of low temperature, however, iron is represented among the lines in the spectrum, but at the highest temperature all visible indications of its presence seem to have vanished.

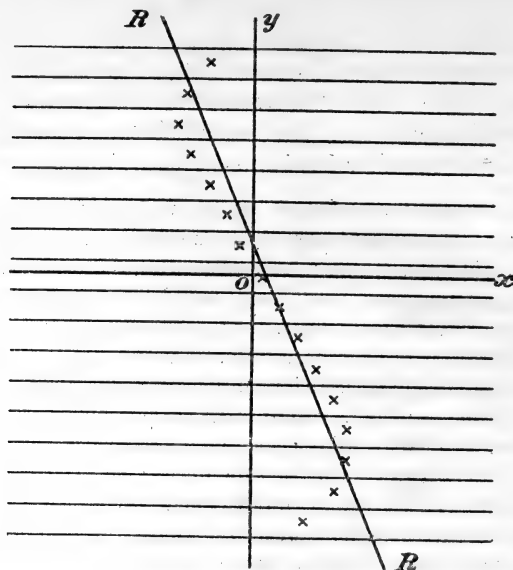
This result affords a valuable confirmation of my view, that the arc spectrum of the metallic elements is produced by molecules of different complexities, and it also indicates that the temperature of the hottest stars is sufficient to produce simplifications beyond those which have so far been produced in our laboratories.

“On the Significance of Bravais' Formulæ for Regression, &c., in the case of Skew Correlation.” By G. UDN YULE.
Communicated by Professor KARL PEARSON, F.R.S.
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The only theory of correlation at present available for practical use is based on the normal law of frequency, but, unfortunately, this law is not valid in a great many cases which are both common and important. It does not hold good, to take examples from biology, for statistics of fertility in man, for measurements on flowers, or for weight measurements even on adults. In economic statistics, on the other hand, normal distributions appear to be highly exceptional: variation of wages, prices, valuations, pauperism, and so forth, are always skew. In cases like these we have at present no means of measuring the correlation by one or more “correlation coefficients” such as are afforded by the normal theory.

It seems worth while noting, under these circumstances, that in ordinary practice statisticians never concern themselves with the form of the correlation, normal or otherwise, but yet obtain results of interest—though always lacking in numerical exactness and frequently in certainty. Suppose the case to be one in which two variables are varying together in time, curves are drawn exhibiting the history of the two. If these two curves appear, generally speaking, to rise and fall together, the variables are held to be correlated. If on the other hand it is not a case of variation with time, the associated pairs may be tabulated in order according to the magnitude of one variable, and then it may be seen whether the entries of the other variable also occur in order. Both methods are of course very rough, and will only indicate very close correlation, but they contain, it seems to me, the point of prime importance at all events with regard to economic statistics. In all the classical examples of statistical correlation (*e.g.*, marriage-rate and imports, corn prices and vagrancy, out-relief and wages) we are only primarily concerned with the question is a large x usually associated with a large y (or small y); the further question as to the form of this association and the relative frequency of different pairs of the variables is, at any rate on a first investigation, of comparatively secondary importance.

Let Ox , Oy be the axes of a *three dimensional* frequency-surface drawn through the mean O of the surface parallel to the axes of measurement, and let the points marked (\bar{x}) be the means of successive x -arrays, lying on some curve that may be called the curve of regression of x on y . Now let a line, RR , be fitted to this curve,



subjecting the distances of the means from the line to some minimal condition. If the slope of RR is positive we may say that large values of x are on the whole associated with large values of y , if it is negative large values of x are associated with small values of y . Further, if the slope of RR to the vertical be given we shall have a measure of a rough practical kind of the shift of the mean of an x -array when its type y is altered. The equation to RR consequently gives a concise and definite answer to two most important statistical questions. It is also evident that if the means of the arrays actually lie in a straight line (as in normal correlation), the equation to RR must be the equation to the line of regression.

Let n be the number of observations in any x -array, and let d be the horizontal distance of the mean of this array from the line RR . I propose to subject the line to the condition that the sum of all quantities like nd^2 shall be a minimum, i.e., I shall use the condition of least squares. I do this solely for convenience of analysis; I do not claim for the method adopted any peculiar advantage as regards the probability of its results. It would, in fact, be absurd to do so, for I am postulating at the very outset that the curve of regression is only exceptionally a straight line; there can consequently be no meaning in seeking for the *most probable* straight line to represent the regression.

Let x, y be a pair of associated deviations, let σ be the standard deviation of any array about its own mean, and let

$$X = a + bY$$

be the equation to RR. Then for any one array

$$S\{x - (a + by)\}^2 = S\{x - (a + bY)\}^2 = n\sigma^2 + nd^2.$$

Hence, extending the meaning of S to summation over the whole surface

$$S(nd^2) = S\{x - (a + by)\}^2 - S n \sigma^2.$$

But in this expression $S(n\sigma^2)$ is independent of a and b , it is, in fact, a characteristic of the surface. Therefore, making $S(nd^2)$ a minimum is equivalent to making

$$S\{x - (a + by)\}^2$$

a minimum. That is to say, we may regard our method in another light. We may say that we form a *single-valued* relation

$$x = a + by$$

between a pair of associated deviations, such that the sum of the squares of our errors in estimating any one x from its y by the relation is a minimum. This single-valued relation, which we may call the characteristic relation, is simply the equation to the line of regression RR. There will be two such equations to be formed corresponding to the two lines of regression.

The idea of the method may at once be extended to the case of correlation between several variables x_1, x_2, x_3 , &c. Let n be the number of observations in an array of x_1 's associated with fixed values X_2, X_3, X_4 , &c., of the remaining variables, let σ_1 be the standard deviation of this array, and let d be the difference of its mean from the value given by a regression equation

$$X_1 = a_{12}X_2 + a_{13}X_3 + a_{14}X_4 + \dots$$

Then, as before, we shall determine the coefficients a_{12}, a_{13}, a_{14} , &c., so as to make Snd^2 a minimum. But this is again equivalent to making

$$S\{x_1 - (a_{12}x_2 + a_{13}x_3 + a_{14}x_4 + \dots)\}^2$$

a minimum for

$$S\{x_1 - (a_{12}x_2 + a_{13}x_3 + a_{14}x_4 + \dots)\}^2 = S(n\sigma_1^2) + S(nd^2).$$

Hence, we may say that we solve for a single-valued relation

$$x_1 = a_{12}x_2 + a_{13}x_3 + a_{14}x_4 + \dots$$

between our variables; the relation being such that the sum of the squares of the errors made in estimating x_1 from its associated values x_2, x_3 , &c., is the least possible. In the case of normal correla-

tion this "characteristic relation" must become the "equation of regression" which gives the means of any x_1 -array, as only in this way can Snd^2 be made a minimum, *i.e.*, zero.

It might be said that it would be more natural to form a "characteristic relation" between the absolute values of the variables and not their deviations from the mean. This may, however, be most conveniently done by working with the *mean* as origin until the characteristic is obtained, and then transferring the equation to zero as origin. It would be much more laborious and would only lead to the same result if zero were used *ab initio* as origin.

We may now proceed to the discussion of the special cases of two, three, or more variables. The actual formulæ obtained are not, it will be found, novel in themselves, but throw an unexpected light on the meaning of the expressions previously given by Bravais* for the case of normal correlation.

(1) *Case of Two Variables*.—Since x and y represent deviations from their respective means, we have, using S to denote summation over the whole surface,

$$S(x) = S(y) = 0.$$

The characteristic or regression equations which we have to find are of the form

$$\left. \begin{aligned} x &= a_1 + b_1 y \\ y &= a_2 + b_2 x \end{aligned} \right\} \dots\dots\dots (1).$$

Taking the equation for x first, the normal equations for a_1 and b_1 are

$$\left. \begin{aligned} S(x) &= Na_1 + b_1 S(y) \\ S(xy) &= a_1 S(y) + b_1 S(y^2) \end{aligned} \right\} \dots\dots\dots (2),$$

N being the total number of correlated pairs. From the first of these equations we have at once

$$a_1 = 0.$$

From the second

$$b_1 = \frac{S(xy)}{S(y^2)}.$$

To simplify our notation let us write

$$\begin{aligned} S(x^2) &= N\sigma_1^2. & S_y &= N\sigma_2^2. \\ S(xy) &= Nr\sigma_1\sigma_2. \end{aligned}$$

σ_1 and σ_2 are then the two standard-deviations or errors of mean

* "Mémoires par divers Savants," 1846, p. 255, and Professor Pearson's paper on "Regression, Heredity, &c." 'Phil. Trans.,' A, vol. 187 (1896), p. 261 *et seq.*

square. r is Bravais' value of the coefficient of correlation. Re-writing b_1 in terms of these symbols, we have

$$b_1 = r \frac{\sigma_1}{\sigma_2} \dots\dots\dots (3)$$

Similarly, $a_2 = 0$, $b_2 = r \frac{\sigma_2}{\sigma_1} \dots\dots\dots (4)$

But the expressions on the right of (3) and (4) are the values obtained by Bravais on the assumption of normal correlation for the regression of x on y , and the regression of y on x . That is to say, the Bravais values for the regressions are simply those values of b_1 and b_2 , which make

$$S(x - b_1 y)^2 \text{ and } S(x - b_2 y)^2$$

respectively minima, whatever be the form of the correlation between the two variables. Again, whatever the form of the correlation, if the regression be really linear, the equations to the lines of regression are those given above (as we pointed out in the introduction). This theorem admits of a very simple and direct geometrical proof.

Let n be the number of correlated pairs in any one array taken parallel to the axis of x , and let θ be the angle that the line of regression makes with the axis of y . Then, for a single array,

$$S(xy) = yS(x) = ny^2 \tan \theta,$$

or extending the significance of S to summation over the whole surface,

$$S(xy) = N \tan \theta \sigma_2^2,$$

that is,

$$\tan \theta = r \frac{\sigma_1}{\sigma_2}.$$

In any case, then, where the regression appears to be linear, Bravais' formulæ may be used at once without troubling to investigate the normality of the distribution. The exponential character of the surface appears to have nothing whatever to do with the result.

To return, again, to the most general case, we see that both coefficients of regression must have the same sign, namely, the sign of r . Hence, either regression will serve to indicate whether there is correlation or no, for there is no reason, *a priori*, why the values of b_1 and b_2 , as determined above, should be positive rather than negative. But, nevertheless, the regressions are not convenient measures of correlation, for, on comparing two similar cases, we may find, say,

$$b_1 > b'_1, \quad b_2 < b'_2,$$

where b_1b_2 , $b'_1b'_2$ are the regressions in the two cases. To which distribution are we, in such a case, to attribute the greater correlation? Bravais' coefficient solves the difficulty, we may say, in one way, by taking the geometrical mean of the two regressions as the measure of correlation. It will still remain valid for non-normal correlation. But there are other and less arbitrary interpretations even in the general case.

Suppose that instead of measuring x and y in arbitrary units we measure each in terms of its own standard deviation. Then let us write

$$\frac{x}{\sigma_1} = \rho \frac{y}{\sigma_2} \dots\dots\dots (5),$$

and solve for ρ by the method of least squares. We have omitted a constant on the right-hand side, since it would vanish as before. We have, at once,

$$\rho = \frac{S(xy)}{S(y^2)} \frac{\sigma_2}{\sigma_1} = r \dots\dots\dots (6),$$

That is to say, if we measure x and y each in terms of its own standard deviation, r becomes at once the regression of x on y , and the regression of y on x . The regressions being, in fact, the fundamental physical quantities, r is a coefficient of correlation because it is a coefficient of regression.*

Again, let us form the sums of the squares of residuals in equations (1) and (5). Inserting the values of b_1 , b_2 , and ρ , we have—

$$\left. \begin{aligned} S(x - b_1y)^2 &= N\sigma_1^2(1 - r^2) \\ S(y - b_2x)^2 &= N\sigma_2^2(1 - r^2) \\ S\left(\frac{x}{\sigma_1} - \rho \frac{y}{\sigma_2}\right)^2 &= S\left(\frac{y}{\sigma_2} - \rho \frac{x}{\sigma_1}\right)^2 = N(1 - r^2) \end{aligned} \right\} \dots\dots\dots (7).$$

Any one of these quantities, being the sum of a series of squares, must be positive. Hence r cannot be greater than unity. If r be equal to unity, or if the correlation be perfect, all the above three sums become zero. But

$$S\left(\frac{x}{\sigma_1} \pm \frac{y}{\sigma_2}\right)^2$$

can only vanish if

$$\frac{x}{\sigma_1} \pm \frac{y}{\sigma_2} = 0$$

in every case, or if the relation hold good,

* That the regression becomes the coefficient of correlation when each deviation is measured in terms of its standard-deviation in the case of normal correlation has been pointed out by Mr. Francis Galton. *Vide* Pearson 'Phil. Trans.,' A, vol. 187, p. 307, note.

$$\frac{x_1}{y_1} = \frac{x_2}{y_2} = \frac{x_3}{y_3} = \dots = \pm \frac{\sigma_1}{\sigma_2} \dots \dots \dots (8);$$

the sign of the last term depending on the sign of r . Hence the statement that two variables are "perfectly correlated" implies that relation (8) holds good, or that all pairs of deviations bear the same ratio to one another. It follows that in correlation, where the means of arrays are not collinear, or the deviation of the mean of the array is not a linear function of the deviation of the type, r can never be unity, though we know from experience that it can approach pretty closely to that value. If the regression be very far from linear, some caution must evidently be used in employing r to compare two different distributions.

In the case of normal correlation, $\sigma_1\sqrt{1-r^2}$ is the standard deviation of any array of the x variables, corresponding to a single type of y 's. $\sigma_2\sqrt{1-r^2}$ is similarly the standard deviation of any array of the y variables, corresponding to a single type of x 's. In the general case, the first expression may be interpreted as the mean standard deviation of the x -arrays from the line of regression, and the second expression as the mean standard deviation of the y -arrays from the line of regression. Otherwise we may regard

$$\sigma_1\sqrt{1-r^2}$$

as the standard error made in estimating x from the relation

$$x = b_1y,$$

and

$$\sigma_2\sqrt{1-r^2}$$

as the standard error made in estimating y from the relation

$$y = b_2x,$$

these interpretations being independent of the form of the correlation.

(2.) Case of Three Variables.

Let the three correlated variables be X_1, X_2, X_3 , and let x_1, x_2, x_3 denote deviations of these variables from their respective means. Let us write, for brevity,

$$S(x_1^2) = N\sigma_1^2, \quad S(x_2^2) = N\sigma_2^2$$

$$S(x_3^2) = N\sigma_3^2$$

$$S(x_1x_2) = Nr_{12}\sigma_1\sigma_2$$

$$S(x_2x_3) = Nr_{23}\sigma_2\sigma_3$$

$$S(x_3x_1) = Nr_{31}\sigma_3\sigma_1.$$

Our characteristic or regression-equation will now be of the form

$$x_1 = b_{12}x_2 + b_{13}x_3 \dots\dots\dots (9),$$

b_{12} and b_{13} being the unknowns to be determined from the observations by the method of least squares. I have omitted a constant term on the right-hand side, since its least-square value would be zero as before. The two normal equations are now—

$$S(x_1x_2) = b_{12}S(x_2^2) + b_{13}S(x_2x_3)$$

$$S(x_1x_3) = b_{12}S(x_2x_3) + b_{13}S(x_3^2),$$

or replacing the sums by the symbols defined above, and simplifying—

$$\left. \begin{aligned} r_{12}\sigma_1 &= b_{12}\sigma_2 + b_{13}r_{23}\sigma_3 \\ r_{13}\sigma_1 &= b_{12}r_{23}\sigma_2 + b_{13}\sigma_3 \end{aligned} \right\} \dots\dots\dots (10),$$

whence

$$\left. \begin{aligned} b_{12} &= \frac{r_{12} - r_{13}r_{23}}{1 - r_{23}^2} \frac{\sigma_1}{\sigma_2} \\ b_{13} &= \frac{r_{13} - r_{12}r_{23}}{1 - r_{23}^2} \frac{\sigma_1}{\sigma_3} \end{aligned} \right\} \dots\dots\dots (11).$$

That is, the characteristic relation between x_1 and x_2x_3 is—

$$x_1 = \frac{r_{12} - r_{13}r_{23}}{1 - r_{23}^2} \frac{\sigma_1}{\sigma_2} x_2 + \frac{r_{13} - r_{12}r_{23}}{1 - r_{23}^2} \frac{\sigma_1}{\sigma_3} x_3 \dots\dots\dots (12).$$

Now Bravais showed that if the correlation were normal, and we selected a group or array of X_1 's with regard to special values h_2 and h_3 of x_2 and x_3 , then h_1 being the deviation of the mean of the selected X_1 's from the X_1 -mean of the whole material,

$$h_1 = b_{12}h_2 + b_{13}h_3,$$

where b_{12} and b_{13} have the values given in (11). But evidently the relation is of much greater generality; it holds good so long as h_1 is a linear function of h_2 and h_3 , *whatever be the law of frequency*.

Further, the values of b_{12} and b_{13} above determined, are, under any circumstances, such that

$$Sv^2 = S[x_1 - (b_{12}x_2 + b_{13}x_3)]^2,$$

is a minimum. If we insert in this expression the values of b_{12} and b_{13} from (11), we have, after some reduction,

$$\begin{aligned} S(v^2) &= N\sigma_1^2 \left\{ 1 - \frac{r_{12}^2 + r_{13}^2 - 2r_{12}r_{23}r_{31}}{1 - r_{23}^2} \right\} \\ &= N\sigma_1^2 \{ 1 - R_1^2 \} \dots\dots\dots (13), \end{aligned}$$

say. In normal correlation $\sigma_1 \sqrt{1-R_1^2}$ is the standard deviation of an X_1 -array, corresponding to any given types of X_2 and X_3 . In general correlation it may be regarded as the mean standard deviation of the X_1 -arrays from the plane

$$x_1 = b_{12}x_2 + b_{13}x_3,$$

or as the standard error made in estimating x_1 from x_2 and x_3 by relation (12).

The quantity R is of some interest, as it exactly takes the place of r in the residual expressions (7). R_1 may, in fact, be regarded as a coefficient of correlation between x_1 and (x_2x_3) ; it can only be unity if the linear relation (9) or (12) hold good in every case.

The quantities b_{12} , b_{13} , &c. (the others may be written down by symmetry), may be termed the net regressions of x_1 on x_2 , x_1 on x_3 , &c. If we write 2 for 1 and 1 for 2 in the value of b_{12} , we have

$$b_{21} = \frac{r_{12} - r_{13}r_{23}}{1 - r_{23}^2} \frac{\sigma_2}{\sigma_1},$$

b_{21} being the the net regression of x_2 on x_1 . In normal correlation, b_{12} and b_{21} are the regressions for any group of X_1 's or X_2 's associated with a fixed type of X_3 's. Hence, in this case (normal correlation), the coefficient of correlation for such a group is the geometrical mean of the two regressions, or

$$\rho_{12} = \frac{r_{12} - r_{13}r_{23}}{\sqrt{(1 - r_{13}^2)(1 - r_{23}^2)}}$$

a quantity that may be called the net coefficient of correlation between x_1 and x_2 .* The similar net coefficients between x_1 and x_3 , x_2 and x_3 , may be written down by interchanging the suffixes.

In normal correlation ρ_{12} is quite strictly the coefficient of correlation for any sub-group of X_1 's and X_2 's, whatever the associated type of X_3 's. In generalised correlation this will not be so, and ρ_{12} can only retain an average significance.

The method does not appear to be capable of investigating changes in the net coefficient as we pass from one type to another, but it may be noted that whatever the form of the correlation, ρ_{12} retains three of the chief properties of the ordinary coefficients: (1) it can only be

* My quantities, b_{12} , b_{13} , &c., were termed by Professor Pearson ("Regression &c.," 'Phil. Trans.,' A, vol. 187 (1896), p. 287), "Coefficients of double regression," and quantities like $b_{12} \frac{\sigma_2}{\sigma_1}$, $b_{13} \frac{\sigma_3}{\sigma_1}$, &c., "coefficients of double correlation." My quantities ρ he did not use. Having named the ρ 's "net correlation," it seemed most natural to rename the b 's "net regressions," as the b 's and ρ 's are corresponding quantities.

Some of my results given above were quoted by Professor Pearson in his paper (*loc. cit.*, notes on pp. 268 and 287).

zero if both net regressions are zero; (2) it is a symmetrical function of the variables; (3) it cannot be greater than unity; for, by (13),

$$r_{12}^2 + r_{13}^2 - 2r_{12}r_{23}r_{31} < 1 - r_{23}^2,$$

or adding $r_{13}^2 r_{23}^2$ to both sides, and transferring r_{13}^2 to the right-hand side

$$(r_{12} - r_{13}r_{23})^2 < (1 - r_{13}^2)(1 - r_{23}^2).$$

If any two coefficients, say $r_{12}r_{13}$, be supposed known, the inequality we have used above will give us limits for the value of the third. Throwing it into the form

$$(r_{23} - r_{12}r_{13})^2 < 1 + r_{12}^2 r_{13}^2 - r_{12}^2 - r_{13}^2,$$

we have r_{23} must lie between the limits

$$r_{12}r_{13} \pm \sqrt{r_{12}^2 r_{13}^2 - r_{12}^2 - r_{13}^2 + 1}.$$

The values of these limits for some special cases are collected in the following table:—

Values of r_{12} and r_{13} .	Limits of r_{23} .
$r_{12} = r_{13} = 0$	0
$r_{12} = r_{13} = \pm 1$	± 1
$r_{12} = +1, r_{13} = -1$	-1
$r_{12} = 0, r_{13} = \pm 1$	0
$r_{12} = 0, r_{13} = \pm r$	$\pm \sqrt{1 - r^2}$
$r_{12} = r_{13} = \pm r$	1 and $2r^2 - 1$
$r_{12} = +r, r_{13} = -r$	$2r^2 - 1$ and -1
$r_{12} = r_{13} = \pm \sqrt{0.5} = 0.707$	0 and 1
$r_{12} = +\sqrt{0.5} \quad r_{13} = -\sqrt{0.5}$	0 „ -1

One is rather prone to argue that if A be correlated with B, and B with C, A will be correlated with C. Evidently this is not necessary. A may be positively correlated with B, and B positively correlated with C, but yet A may, in general, be negatively correlated with C. Only, if the coefficients (AB) and (BC) are both numerically greater than 0.707, can one even ascribe the correct sign to the (AC) correlation.

It is evident that one would, in general, expect to make a smaller standard error in estimating x_1 from the two associated variables x_2 and x_3 , than in estimating it from one only, say x_2 . But it seems desirable to prove this specifically, and to investigate under what conditions it will hold good. The necessary condition is—

$$\frac{r_{12}^2 + r_{13}^2 - 2r_{12}r_{23}r_{13}}{1 - r_{23}^2} > r_{12}^2,$$

that is,

$$r_{12}^2 + r_{13}^2 - 2r_{12}r_{13}r_{23} > r_{12}^2 - r_{12}^2 r_{13}^2,$$

or

$$(r_{13} - r_{12}r_{23})^2 > 0.$$

But $(r_{13} - r_{12}r_{23})$ is the numerator of ρ_{13} , the net coefficient of correlation between x_1 and x_3 . Hence the standard error in the second case will be always less than in the first, so long as ρ_{13} is not zero. The condition is somewhat interesting.

To take an arithmetical example, suppose one had in some actual case

$$r_{12} = +0.8$$

$$r_{23} = +0.5 \quad r_{13} = +0.4.$$

One might very naturally imagine that the introduction of the third variable with a fairly high correlation coefficient (0.4) would considerably lessen the standard deviation of the x_1 -array; but this is not so; for

$$\rho_{13} = \frac{0.4 - (0.5 \times 0.8)}{\sqrt{0.75 \times 0.36}} = 0,$$

so the third variable would be of no assistance.

III. Case of Four Variables.

This case is, perhaps, of sufficient practical importance to warrant our developing the results at length as in the last.

If x_1, x_2, x_3, x_4 , be the associated deviations of the four variables from their respective means, the characteristic equation will be of the form

$$x_1 = b_{12}x_2 + b_{13}x_3 + b_{14}x_4 \dots \dots \dots (14).$$

The normal equations for the b 's are, in our previous notation,

$$\left. \begin{aligned} r_{12}\sigma_1 &= b_{12}\sigma_2 + b_{13}r_{23}\sigma_3 + b_{14}r_{24}\sigma_4 \\ r_{13}\sigma_1 &= b_{12}r_{23}\sigma_2 + b_{13}\sigma_3 + b_{14}r_{34}\sigma_4 \\ r_{14}\sigma_1 &= b_{12}r_{24}\sigma_2 + b_{13}r_{34}\sigma_3 + b_{14}\sigma_4 \end{aligned} \right\}$$

Hence

$$b_{12} = \frac{\begin{vmatrix} r_{13} & r_{23} & r_{24} \\ r_{14} & r_{34} & 1 \end{vmatrix}}{\begin{vmatrix} 1 & r_{23} & r_{24} \\ r_{23} & 1 & r_{34} \\ r_{24} & r_{34} & 1 \end{vmatrix}} \frac{\sigma_1}{\sigma_2} \dots \dots \dots (15),$$

and so on for the others, b_{13}, b_{14} , &c., we may call the net regressions of x_1 on x_2, x_1 on x_3 , &c., as before. By parity of notation, we have

$$b_{21} = \frac{\begin{vmatrix} r_{13} & r_{23} & r_{24} \\ r_{13} & 1 & r_{34} \\ r_{14} & r_{34} & 1 \end{vmatrix}}{\begin{vmatrix} 1 & r_{13} & r_{14} \\ r_{13} & 1 & r_{34} \\ r_{14} & r_{34} & 1 \end{vmatrix}} \frac{\sigma_2}{\sigma_1}$$

and we may again call

$$\rho_{12} = \sqrt{b_{12}b_{21}},$$

the net coefficient of correlation between x_1 and x_2 . Expanding the determinants, we have, in fact,

$$\rho_{12} = \frac{r_{12}(1-r_{34}^2) + r_{13}(r_{34}r_{24}-r_{23}) + r_{14}(r_{23}r_{34}-r_{24})}{\sqrt{[(1-r_{34}^2) + r_{23}(r_{34}r_{24}-r_{23}) + r_{24}(r_{23}r_{34}-r_{24})][(1-r_{34}^2) + r_{13}(r_{34}r_{14}-r_{13}) + r_{14}(r_{13}r_{34}-r_{14})]}} \dots\dots\dots (16).$$

There are six such net coefficients, ρ_{12} , ρ_{13} , ρ_{14} , ρ_{23} , ρ_{24} , ρ_{34} . The above values of the regressions are again those usually obtained on the assumption of normal correlation.* The net correlation ρ_{12} becomes, on that assumption, the coefficient of correlation for any group of the x_1 x_2 variables associated with fixed types of x_3 and x_4 . If we write

$$u = x_1 - (b_{12}x_2 + b_{13}x_3 + b_{14}x_4),$$

we have, after some rather lengthy reduction,

$$\frac{1}{N} S(u^2) = \sigma_1^2(1-R_1^2),$$

where

$$R_1^2 = \frac{\left\{ \begin{aligned} &r_{12}^2 + r_{13}^2 + r_{14}^2 - r_{12}^2 r_{34}^2 - r_{23}^2 r_{14}^2 - r_{13}^2 r_{24}^2 \\ &- 2(r_{13}r_{14}r_{34} + r_{12}r_{14}r_{24} + r_{12}r_{13}r_{23}) + 2(r_{12}r_{14}r_{23}r_{34} + r_{13}r_{14}r_{23}r_{24} + r_{12}r_{13}r_{24}r_{34}) \end{aligned} \right\}}{1 - r_{23}^2 - r_{34}^2 - r_{24}^2 + 2r_{23}r_{34}r_{24}}.$$

In normal correlation, $\sigma_1\sqrt{1-R_1^2}$ is the standard deviation of all x_1 -arrays associated with fixed types of x_2 , x_3 , and x_4 . In general correlation, it is most easily interpreted as the standard error made in estimating x_1 , by equation (14), from its associated values of x_2 , x_3 , and x_4 .

As in the case of three variables, the quantity R may be considered as a coefficient of correlation. It can range between ± 1 , and can only become unity if the linear relation (14) hold good in each individual instance.

We showed at the end of the last section that the standard error made in estimating x_1 from the relation

$$x_1 = b_{12}x_2 + b_{13}x_3$$

* Professor Pearson, "Regression, Heredity, and Panmixia." 'Phil. Trans.,' A, vol. 187 (1896), p. 294.

was always less than the standard error when only x_2 was taken into account, unless

$$\rho_{13} = 0.$$

We may now prove the similar theorem that when we use three variables, x_2, x_3, x_4 , on which to base the estimate, the standard error will be again decreased, unless

$$\rho_{14} = 0.$$

The condition that $S(u^2)$, in our present case, shall be less than $S(r^2)$ in the last, is, in fact,

$$\left\{ \begin{aligned} & r_{12}^2 + r_{13}^2 + r_{14}^2 - r_{12}^2 r_{34}^2 - r_{23}^2 r_{14}^2 - r_{13}^2 r_{24}^2 \\ & - 2(r_{13} r_{14} r_{34} + r_{12} r_{13} r_{23} + r_{12} r_{14} r_{24}) \\ & + 2(r_{12} r_{14} r_{23} r_{34} + r_{14} r_{13} r_{24} r_{23} + r_{12} r_{13} r_{24} r_{34}) \end{aligned} \right\} (1 - r_{23}^2) \\ > (r_{12}^2 + r_{13}^2 - 2r_{12} r_{13} r_{23})(1 - r_{23}^2 - r_{24}^2 - r_{34}^2 + 2r_{23} r_{24} r_{34}).$$

This may be finally reduced to—

$$(r_{14} - r_{13} r_{34} - r_{12} r_{24} - r_{14} r_{23}^2 + r_{13} r_{23} r_{24} + r_{12} r_{23} r_{34})^2 > 0,$$

that is

$$\rho_{14}^2 > 0.$$

The treatment of the general case of n variables, so far as regards obtaining the regressions, is obvious, and it is unnecessary to give it at length.

We can now see that the use of normal regression formulæ is quite legitimate in all cases, so long as the necessary limitations of interpretation are recognised. Bravais' r always remains a coefficient of correlation. These results I must plead as justification for my use of normal formulæ in two cases* where the correlation was markedly non-normal.

“Mathematical Contributions to the Theory of Evolution.—On a Form of Spurious Correlation which may arise when Indices are used in the Measurement of Organs.” By KARL PEARSON, F.R.S., University College, London. Received December 29, 1896,—Read February 18, 1897.

(1) If the ratio of two absolute measurements on the same or different organs be taken it is convenient to term this ratio an *index*.

If $u = f_1(x, y)$ and $v = f_2(z, y)$ be two functions of the three variables x, y, z , and these variables be selected at random so that there exists no correlation between x, y, y, z , or z, x , there will still be found to

* ‘Economic Journal,’ Dec., 1895, and Dec., 1896, “On the Correlation of Total Pauperism with Proportion of Out-relief.”

exist correlation between u and v . Thus a real danger arises when a statistical biologist attributes the correlation between two functions like u and v to *organic* relationship. The particular case that is likely to occur is when u and v are indices with the same denominator for the correlation of indices seems at first sight a very plausible measure of organic correlation.

The difficulty and danger which arise from the use of indices was brought home to me recently in an endeavour to deal with a considerable series of personal equation data. In this case it was convenient to divide the errors made by three observers in estimating a variable quantity by the actual value of the quantity. As a result there appeared a high degree of correlation between three series of absolutely independent judgments. It was some time before I realised that this correlation had nothing to do with the manner of judging, but was a special case of the above principle due to the use of indices.

A further illustration is of the following kind. Select three numbers within certain ranges at random, say x, y, z , these will be pair and pair uncorrelated. Form the proper fractions x/y and z/y for each triplet, and correlation will be found between these indices.

The application of this idea to biology seems of considerable importance. For example, a quantity of bones are taken from an *ossuarium*, and are put together in groups, which are asserted to be those of individual skeletons. To test this a biologist takes the triplet femur, tibia, humerus, and seeks the correlation between the indices *femur/humerus* and *tibia/humerus*. He might reasonably conclude that this correlation marked organic relationship, and believe that the bones had really been put together substantially in their individual grouping. As a matter of fact, since the coefficients of variation for femur, tibia, and humerus are approximately equal, there would be, as we shall see later, a correlation of about 0.4 to 0.5 between these indices had the bones been sorted absolutely at random. I term this a spurious organic correlation, or simply a spurious correlation. I understand by this phrase the amount of correlation which would still exist between the indices, were the absolute lengths on which they depend distributed at random.

It has hitherto been usual to measure the organic correlation of the organs of shrimps, prawns, crabs, &c., by the correlation of indices in which the denominator represents the total body length or total carapace length. Now suppose a table formed of the absolute lengths and the indices of, say, some thousand individuals. Let an "imp" (allied to the Maxwellian demon) redistribute the indices at random, they would then exhibit no correlation; if the corresponding absolute lengths followed along with the indices in the redistribution, they also would exhibit no correlation. Now let us suppose the indices not to have been calculated, but the imp to redistribute the abso-

lute lengths; these would now exhibit no organic correlation, but the indices calculated from this random distribution would have a correlation nearly as high, if not in some cases higher than before. The biologist would be not unlikely to argue that the index correlation of the imp-assorted, but probably, from the vital standpoint, impossible beings was "organic."

As a last illustration, suppose 1000 skeletons obtained by distributing component bones at random. Between none of their bones will these individuals exhibit correlation. Wire the spurious skeletons together and photograph them all, so that their stature in the photographs is the same; the series of photographs, if measured, will show correlation between their parts. It seems to me that the biologist who reduces the parts of an animal to fractions of some one length measured upon it is dealing with a series very much like these photographs. A part of the correlation he discovers between organs is undoubtedly organic, but another part is solely due to the nature of his arithmetic, and as a measure of organic relationship is spurious.

Returning to our problem of the randomly distributed bones, let us suppose the indices femur/humerus and tibia/humerus to have a correlation of 0.45. Now suppose successively 1, 2, 3, 4, &c., per cent. of the bones are assorted in their true groupings, then begins the true organic correlating of the bones. It starts from 0.45, and will alter gradually until 100 per cent. of the bones are truly grouped. The final value may be greater or less than 0.45, but it would seem that 0.45 is a more correct point to measure the organic correlation from than zero. At any rate it appears fairly certain that if a biologist recognised that a perfectly random selection of organs would still lead to a correlation of organ-indices, he would be unlikely to accept index-correlation as a fair measure of the relative intensity of correlation between organs. I shall accordingly define spurious organic correlation as the correlation which will be found between indices, when the absolute values of the organs have been selected purely at random. In estimating relative correlation by the hitherto usual measurement of indices, it seems to me that a statement of the amount of spurious correlation ought always to be made.

(2) *Proposition I.—To find the mean of an index in terms of the means, coefficients of variation, and coefficient of correlation of the two absolute measurements.**

Let x_1, x_2, x_3, x_4 be the absolute sizes of any four correlated organs; m_1, m_2, m_3, m_4 their mean values; $\sigma_1, \sigma_2, \sigma_3, \sigma_4$ their standard deviations;

* In all that follows, unless otherwise stated, the correlation may be of any kind whatever, i.e., the frequencies are not supposed to follow the Gaussian or normal law of error.

v_1, v_2, v_3, v_4 their coefficients of variation, i.e., $\sigma_1/m_1, \sigma_2/m_2, \sigma_3/m_3, \sigma_4/m_4$ respectively; $r_{12}, r_{23}, r_{34}, r_{41}, r_{24}, r_{13}$, the six coefficients of correlation; $\epsilon_1, \epsilon_2, \epsilon_3, \epsilon_4$ the deviations of the four organs from their means, i.e., $x_1 = m_1 + \epsilon_1, x_2 = m_2 + \epsilon_2, x_3 = m_3 + \epsilon_3, x_4 = m_4 + \epsilon_4$; i_{13} the mean value of the index x_1/x_3 , and i_{24} the mean value of x_2/x_4 ; Σ_1, Σ_2 the standard deviations of the indices x_1/x_3 and x_2/x_4 respectively; and n the total number of groups of organs.

We shall suppose the ratios of the deviations to the mean absolute values of the organs are so small that their cubes may be neglected.

Then

$$\begin{aligned} i_{13} &= \frac{1}{n} S \left(\frac{x_1}{x_3} \right) = \frac{1}{n} \frac{m_1}{m_3} S \left\{ \left(1 + \frac{\epsilon_1}{m_1} \right) \left(1 + \frac{\epsilon_3}{m_3} \right)^{-1} \right\} \\ &= \frac{1}{n} \frac{m_1}{m_3} \left(n + \frac{S(\epsilon_1)}{m_1} - \frac{S(\epsilon_3)}{m_3} - \frac{S(\epsilon_1 \epsilon_3)}{m_1 m_3} + \frac{S(\epsilon_3^2)}{m_3^2} \right), \end{aligned}$$

if we neglect quantities of the third order in ϵ/m . But $S(\epsilon_1) = S(\epsilon_3) = 0$, $S(\epsilon_1 \epsilon_3) = nr_{13}\sigma_1\sigma_3$, and $S(\epsilon_3^2) = n\sigma_3^2$.

Hence:

$$i_{13} = \frac{m_1}{m_3} (1 + v_3^2 - r_{13}v_1v_3) \dots\dots\dots (i).$$

Similarly

$$i_{24} = \frac{m_2}{m_4} (1 + v_4^2 - r_{24}v_2v_4) \dots\dots\dots (ii).$$

Thus we see that the mean of an index is not the ratio of the means of the corresponding absolute measurements, but differs by a quantity depending on the correlation and variation coefficients of the absolute measurements.

(3) *Proposition II.*—To find the standard deviation of an index in terms of the coefficients of variation, and coefficient of correlation of the two absolute measurements.

$$\begin{aligned} n\Sigma_{13}^2 &= S \left(\frac{x_1}{x_3} - i_{13} \right)^2 \\ &= \frac{m_1^2}{m_3^2} S \left\{ \left(1 + \frac{\epsilon_1}{m_1} \right) \left(1 + \frac{\epsilon_3}{m_3} \right)^{-1} - (1 + v_3^2 - r_{13}v_1v_3) \right\}^2 \\ &= \frac{m_1^2}{m_3^2} S \left\{ \left(\frac{\epsilon_1}{m_1} - \frac{\epsilon_3}{m_3} + \text{square terms} \right)^2 \right\} \\ &= i_{13}^2 (nv_1^2 + nv_3^2 - 2nr_{13}v_1v_3), \end{aligned}$$

if we neglect cubic terms.

$$\therefore \Sigma_{13} = i_{13} \sqrt{(v_1^2 + v_3^2 - 2r_{13}v_1v_3)} \dots\dots\dots (iii).$$

(4) *Proposition III.*—To find the coefficient of correlation of two indices in terms of the coefficients of correlation of the four absolute measurements and their coefficients of variation.

Let x_1/x_3 and x_2/x_4 be the two indices.

$$\begin{aligned} n\rho\Sigma_{13}\Sigma_{24} &= S\left(\frac{x_1}{x_3}-i_{13}\right)\left(\frac{x_2}{x_4}-i_{24}\right) \\ &= \frac{m_1m_2}{m_3m_4}S\left(1+\frac{\epsilon_1}{m_1}-\frac{\epsilon_3}{m_3}-\frac{\epsilon_1\epsilon_3}{m_1m_3}+\frac{\epsilon_3^2}{m_3^2}-1-v_3^2+r_{13}v_1v_3\right) \\ &\quad \times \left(1+\frac{\epsilon_2}{m_2}-\frac{\epsilon_4}{m_4}-\frac{\epsilon_2\epsilon_4}{m_2m_4}+\frac{\epsilon_4^2}{m_4^2}-1-v_4^2+r_{14}v_1v_4\right) \\ &= i_{13}i_{24}S\left(\frac{\epsilon_1}{m_1}-\frac{\epsilon_3}{m_3}\right)\left(\frac{\epsilon_2}{m_2}-\frac{\epsilon_4}{m_4}\right), \end{aligned}$$

if we neglect terms of the cubic order.

$$\therefore \rho\Sigma_{13}\Sigma_{24} = i_{13}i_{24}(r_{12}v_1v_2-r_{14}v_1v_4-r_{23}v_2v_3+r_{34}v_3v_4).$$

Hence, finally,

$$\rho = \frac{r_{12}v_1v_2-r_{14}v_1v_4-r_{23}v_2v_3+r_{34}v_3v_4}{\sqrt{v_1^2+v_3^2-2r_{13}v_1v_3}\sqrt{v_2^2+v_4^2-2r_{24}v_2v_4}} \dots\dots\dots (iv).$$

(5) Thus we have expressed ρ in terms of the four coefficients of correlation and the four coefficients of variation of the absolute measurements which form the indices.

We may draw the following conclusions :

(i.) The correlation between two indices will always vanish when the four absolute measurements forming the indices are quite uncorrelated.

(ii.) If two of the organs are perfectly correlated, let us say made identical: for example, the third and fourth, so that $r_{34} = 1$, and $v_3 = v_4$, we find

$$\rho = \frac{r_{12}v_1v_2-r_{13}v_1v_3-r_{23}v_2v_3+v_3^2}{\sqrt{v_1^2+v_3^2-2r_{13}v_1v_3}\sqrt{v_2^2+v_3^2-2r_{23}v_2v_3}} \dots\dots\dots (v).$$

This is the coefficient of correlation between two indices with the same denominator (x_1/x_3 and x_2/x_3).

The value of ρ in (v) does not vanish if the remaining organs be quite uncorrelated, i.e., $r_{12} = r_{13} = r_{23} = 0$. In this case

$$\rho_0 = \frac{v_3^2}{\sqrt{v_1^2+v_3^2}\sqrt{v_2^2+v_3^2}} \dots\dots\dots (vi).$$

This is the measure of the spurious correlation. For the special

case in which the coefficients of variation are all the same, $\rho_0 = 0.5$. When the absolute sizes of organs are very feebly correlated, then in most cases there will be a considerable correlation of indices.

Example (a). Suppose three organs, x_1 , x_2 , and x_3 to have sensibly equal coefficients of variation, and that the correlation of x_1 and $x_2 = r_{12} = r$ and of x_1 and x_3 , as well as of x_2 and $x_3 = r'$.

Then :

$$\begin{aligned}\rho &= 0.5 \times \frac{1+r-2r'}{1-r'} \\ &= 0.5 + 0.5 \frac{r-r'}{1-r'}.\end{aligned}$$

This formula illustrates well in a specially simple case how the correlation in the indices diverges from the spurious value 0.5, as we alter r and r' from zero, *i.e.*, as we introduce organic correlation. According as r , the correlation of the numerators, is greater or less than r' , the correlation of the numerator with the denominator, the actual index correlation can be greater or less than the spurious value.

Example (b). If z_1 , z_2 be the indices, then in the case of normal correlation the contour lines of the correlation surface for the indices are given by

$$\frac{z_1^2}{\Sigma_1^2(1-\rho^2)} - \frac{2\rho z_1 z_2}{\Sigma_1 \Sigma_2(1-\rho^2)} + \frac{z_2^2}{\Sigma_2^2(1-\rho^2)} = \text{constant},$$

where Σ_1 , Σ_2 , and ρ are given by (iii) and (iv) above.

The contour lines of a surface of spurious index correlation are given by

$$\frac{z_1^2}{\sigma_1^2} \left(1 + \frac{v_3^2}{v_2^2} \right) - 2 \frac{z_1 z_2}{\sigma_1 \sigma_2} \frac{v_3^2}{v_1 v_2} + \frac{z_2^2}{\sigma_2^2} \left(1 + \frac{v_3^2}{v_1^2} \right) = \text{constant},$$

while the uncorrelated distribution of the numerators x_1 and x_2 is given by the contours,

$$x_1^2/\sigma_1^2 + x_2^2/\sigma_2^2 = \text{constant}.$$

We are thus able to mark the growth of the spurious correlation as we increase v_3 from zero ; we see the axes of the ellipses diminishing and their directions beginning to rotate.

Example (c). To find the spurious correlation between the two chief cephalic indices.

I have calculated the following results from the measurements made on 100 "Altbayerisch" ♂ skulls, by Professor J. Ranke. See his 'Anthropologie der Bayern,' Bd. i, Kapitel v, S. 194.

Breadth of skull :*	$m_1 = 150.47,$	$\sigma_1 = 5.8488,$	$v_1 = 3.8871.$
Height of skull :	$m_2 = 133.78,$	$\sigma_2 = 4.6761,$	$v_2 = 3.4954.$
Length of skull :	$m_3 = 180.58,$	$\sigma_3 = 5.8441,$	$v_3 = 3.2363.$
Cephalic index, B/L :	$i_{13} = 83.41,$	$\Sigma_{13} = 3.5794,$	$V_{13} = 4.2913.$
Cephalic index, H/L :	$i_{23} = 74.23,$	$\Sigma_{23} = 3.6305,$	$V_{23} = 4.8909.$
Cephalic index, H/B :	$i_{21} = 89.12,$	$\Sigma_{21} = 4.1752,$	$V_{21} = 4.6849.$

The coefficients of correlation may at once be deduced :

$$\text{Breadth and length : } r_{13} = (v_1^2 + v_3^2 - V_{13}^2)/(2v_1v_3) = 0.2849.$$

$$\text{Height and length : } r_{23} = (v_2^2 + v_3^2 - V_{23}^2)/(2v_2v_3) = -0.0543.$$

$$\text{Height and breadth : } r_{21} = (v_2^2 + v_1^2 - V_{21}^2)/(2v_1v_2) = 0.1243.$$

This is the first table, so far as I am aware, that has been published of the variation and correlation of the three chief cephalic lengths.† It shows us that there is not at all a close correlation between these chief dimensions of the skull, and that a small compensating factor for size is to be sought in the correlation of height and length, *i.e.*, while a broad skull is probably a long skull and also a high skull, a high skull will probably be a short skull, and a low skull a long skull.

Without substituting the values of $v_1, v_2, v_3, r_{12}, r_{13}, r_{23}$ in (v), we can find ρ , or the correlation between breadth/length and height/length indices from :

$$\rho = (V_{13}^2 + V_{23}^2 - V_{12}^2)/(2V_{13}V_{23}).$$

This follows at once from the general theorem given in my memoir on "Regression, Panmixia, and Heredity," 'Phil. Trans.,' vol. 187, A, p. 279, or by substitution of the above values of r_{12}, r_{13}, r_{23} in (v), we find :

$$\rho = 0.4857.$$

If we calculate from (vi) the correlation between the same cephalic indices on the hypothesis that their heights, breadths and lengths are distributed at random, *i.e.*, that our "imp" has constructed a number of arbitrary and spurious skulls from Professor Ranke's measurements, we find :

$$\rho_0 = 0.4008.$$

It seems to me that a quite erroneous impression would be formed of the organic correlation of the human skull, did we judge it by the magnitude of the correlation coefficient (0.4857) for the two chief

* All the absolute measures given are in millimetres, and the coefficients of variation are *percentage* variations, *i.e.*, they must be divided by 100 before being used in formulæ (i), (ii), and (iii).

† I hope later to treat correlation in man with reference to race, sex, and organ, as I have treated variation.

cephalic indices, for no less than 0·4008 of this would remain, if we destroyed all organic relationship between the lengths on which these indices are based.

Example (d). To find the spurious correlation between the indices femur/humerus and femur/tibia.

The following results have been calculated* from measurements made by Koganei on Aino skeletons. (See 'Mittheilungen aus der medicinischen Facultät der K. J. Universität, Tokio,' Bd. I. Tables.)

I have kept the sexes apart although there are but few of each.

♂ Skeletons. Number = 40 to 44. Measurements in centimetres.

Femur, F: $m_1 = 40\cdot845$, $\sigma_1 = 1\cdot957$, $v_1 = 4\cdot792$.

Tibia, T: $m_2 = 31\cdot740$, $\sigma_2 = 1\cdot577$, $v_2 = 4\cdot970$.

Humerus, H: $m_3 = 29\cdot593$, $\sigma_3 = 1\cdot337$, $v_3 = 4\cdot517$.

The following coefficients of correlation were calculated directly:

Femur and tibia: $r_{12} = 0\cdot8286$.

Femur and humerus: $r_{13} = 0\cdot8585$.

Tibia and humerus: $r_{23} = 0\cdot7447$.

From these were deduced by the formulæ of this paper†:—

Index, F/T: $i_{12} = 128\cdot75$, $\Sigma_{12} = 3\cdot7075$, $V_{12} = 2\cdot8795$.

Index, F/H: $i_{13} = 137\cdot92$, $\Sigma_{13} = 3\cdot4084$, $V_{13} = 2\cdot4714$.

Index, T/H: $i_{23} = 107\cdot02$, $\Sigma_{23} = 3\cdot6675$, $V_{23} = 3\cdot4271$.

Hence we find for the correlation of the indices F/H and T/H:

$$\rho = 0\cdot5644.$$

But the spurious correlation, if the bones had been grouped at random would have been

$$\rho_0 = 0\cdot4557.$$

* I have to thank Miss Alice Lee for a considerable part of the arithmetic work of this example.

† The values for the indices are not in absolute agreement with those to be deduced from the lengths, for it was not always possible to use the same skeleton for femur and humerus as for tibia and humerus, *i.e.*, sometimes one or other bone was missing. For the same reason, the constants for the absolute lengths do not agree entirely with those given for Ainos in my paper on "Variation in Man and Woman" in 'The Chances of Death and other Studies in Evolution,' vol. 1, p. 303), for the simple reason that I there used every available bone, and not every available pair, as here.

Tabulating the corresponding quantities for the other sex we find

♀ Skeletons. Number = 22 to 24. Measurements in centimetres.

Femur, F:	$m_1 = 38.075,$	$\sigma_1 = 1.494,$	$v_1 = 3.924.$
Tibia, T:	$m_2 = 29.800,$	$\sigma_2 = 1.576,$	$v_2 = 5.289.$
Humerus, H:	$m_3 = 27.565,$	$\sigma_3 = 1.109,$	$v_3 = 4.022.$
Femur and tibia:	$r_{12} = 0.8457.$		
Femur and humerus:	$r_{13} = 0.8922.$		
Tibia and humerus:	$r_{23} = 0.7277.$		
Index, F/T:	$i_{12} = 127.90,$	$\Sigma_{12} = 3.8937,$	$V_{12} = 3.0444.$
Index, F/H:	$i_{13} = 138.37,$	$\Sigma_{13} = 2.6930,$	$V_{13} = 1.9462.$
Index, T/H:	$i_{23} = 108.36,$	$\Sigma_{23} = 4.1022,$	$V_{23} = 3.7857.$

$$\rho = 0.6006.$$

$$\rho_0 = 0.3904.$$

Hence we may conclude as follows:

(i) The absolute lengths of the long bones differ from those of the skull in being very closely correlated.

(ii) The use of indices for the long bones would appear to minimise, rather than, as in the case of the skull, to exaggerate this correlation.

(iii) If we measure, however, organic correlation of the indices by $\rho - \rho_0$, we shall find index correlation less than absolute length correlation for both long bones and skull, and in both cases the former comparatively small as compared with the latter.

(iv) The results for the 24 female skeletons, although based on but few data, serve on the whole to confirm the male results.*

(6.) From the above examples it will be seen that the method, which judges of the intensity of organic correlation by the reduction of all absolute measures to indices, the denominators of which are some *one* absolute measurement, is not free from obscurity; for this method would give the major portion of the observed index correlation had the parts of the animal been thrown together entirely at random, *i.e.*, if there were no organic correlation at all. The following additional remarks may be of interest. The results (iv)—(vi) show us that the correlation coefficients of indices are functions, not only of the correlation coefficients of absolute measurements, but also of the coefficients of variation of the latter measurements. Hence,

* The fact that the male is more variable in height-sitting, in femur, and in tibia than the female, while she appears to be more variable than he is in stature, led me to prophesy, in my paper on "Variation in Man and Woman," that the female would be found to be more closely correlated in the bones forming stature than the male. This appears to be the case for the femur and tibia of Ainos.

unless the coefficients of variation be constant for local races, it is impossible that the coefficients of correlation can be constant for indices. In other words, the hypothesis of the constancy for local races of correlation, and that of the constancy for local races of variation, stand on exactly the same footing.

The conclusions of this paper although applied to organic correlation are equally valid so far as concerns the use of indices in judging the correlation of either physical or economic phenomena. It was, indeed, a difficulty arising from my discussion of personal judgments—a spurious correlation between the judgments of different observers—which first drew my attention to the matter.

Note, January 13, 1897.—The result described by Professor Pearson evidently affects the value of the correlation coefficients determined by me in *Crangon* and *Carcinus* ('Roy. Soc. Proc.,' vols. 51 and 54), because I have always expressed the size of the organs measured in terms of body length.

In order to show the effect of this, I have lately performed, at Professor Pearson's suggestion, the following experiment: It happens that my measures of Plymouth shrimps are recorded in a book, in the order in which they were measured, and therefore at random as regards carapace length or other characters. I constructed from these records 420 "spurious" shrimps, in the following way: the total length of the first shrimp in the book was associated with the carapace length of the tenth shrimp and the "post-spinous length" of the twentieth, and so throughout. Evidently these three measures were associated at random, and we might expect that these spurious shrimps would show no organic correlation; but when the carapace lengths and "post-spinous lengths" of these spurious shrimps were divided by the body length, and the correlation between the resulting indices was determined, the value of r was found to be 0.38, the value for real shrimps being 0.81, or the correlation due to the use of indices forms 47 per cent. of the observed value.

W. F. R. WELDON.

"Note to the Memoir by Professor Karl Pearson, F.R.S., on Spurious Correlation." By FRANCIS GALTON, F.R.S. Received January 4,—Read February 18, 1897.

I send this note to serve as a kind of appendix to the memoir of Professor K. Pearson, believing that it may be useful in enabling others to realise the genesis of spurious correlation. It is important though rather difficult to do so, because the results arrived at in the memoir, which are of serious interest to practical statisticians, have at first sight a somewhat paradoxical appearance.

The diagrams show how a table of frequency of the various combinations of two independent and normal variables may be changed into one of A/C , B/C , where C is also an independent and normal variable in respect to its intrinsic qualities, but subjected to the condition that the same value of C is to be used as the divisor of *both* members of the same couplet of A and B . In short, that the couplets shall always be of the form A/C_n , B/C_n , and never that of A/C_n , B/C_m .

For the sake of clearness, the simplest possible suppositions, that are at the same time serviceable, will be made in regard to the particular case illustrated by the diagrams, namely, that A , B , and C , severally, are sharply divided into three, and only into three, equal grades of magnitude, distinguished as AI, AII, AIII; BI, BII, BIII; and CI, CII, CIII; also that the frequency with which these three grades occur is expressed by the three terms of the binomial $(1+1)^2$. Consequently there is one occurrence of I to two occurrences of II and to one occurrence of III. Roman and italic figures are here used to keep the distinction clear between magnitudes and frequencies. It will be easily gathered as we proceed, without the need of special explanation, that the smallness of the value of the binomial index has no influence either on the general character of the operation or on its general result.

The large figures in the outlined square, occupying the lower right hand portion of fig. 1, show the distribution of frequency of the various combinations of A and B . The scales running along the top and down the left side of the figure, which are there assigned to the values of A/C , B/C , apply to these entries also. The latter run in the same way as those in Table I below, or when quadrupled, as they will be for purposes immediately to be explained, as in Table II.

Table I.

1	2	1
2	4	2
1	2	1

Table II.

4	8	4
8	16	8
4	8	4

Let us now follow the fortunes of one of the large figures in fig. 1, say that which refers to $A = I$, $B = III$, of which the frequency is only 1. When the latter is expanded into the three possible values of the form A/C , B/C , caused by the three varieties of C , it yields $\frac{1}{4}$ case of frequency to $(I/I, III/I)$, $\frac{2}{4}$ case to $(I/II, III/II)$, and $\frac{1}{4}$ case to $(I/III, III/III)$, for entry at the intersections of the lines (I, III) , $(I/II, III/II)$, and $(I/III, I)$ respectively.

But, in order to avoid the inconvenience of quarter values, it is better to suppose the original figures in the fig. and in Table I above to have been replaced by those in Table II; then the original entry

Fig. 1.

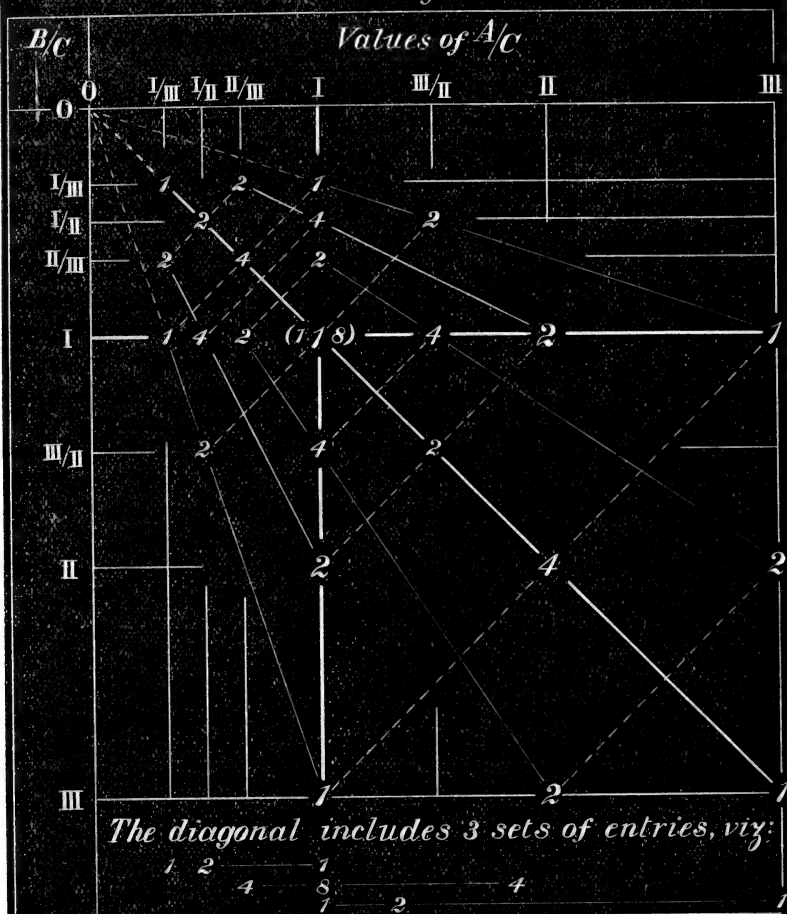
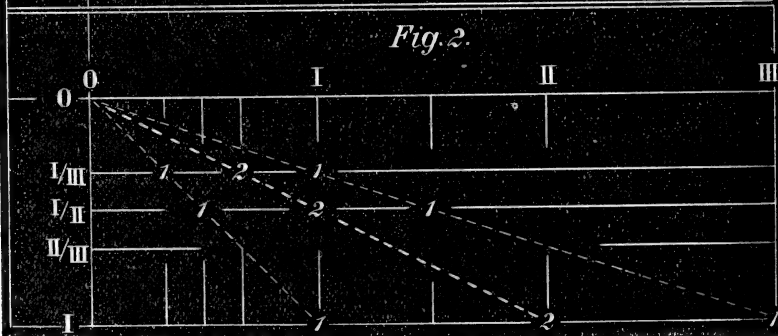


Fig. 2.



from which we start will have become four, to be expanded into three derivative entries, having respectively the frequencies 1, 2, and 1; these latter figures are entered in fig. 1 at the intersections of the lines just named. Under this arrangement the large figure from which we started, which had been changed from 1 to 4, again assumes its original value of 1. It will easily be understood, that the positions of the three derivative entries necessarily lie in the same straight line, and that this line necessarily runs towards the (O, O) corner of the figure. The same is true for every other set of derivative entries, with the result that whereas the original set of large figures, referring to the combinations of A and B, are symmetrically disposed on either side of the horizontal, of the vertical, and of the diagonal lines passing through their common centre at (II, II), the derivative values of A/C, B/C are disposed symmetrically only in respect to the diagonal line that runs from the (O, O) corner. Their symmetry, in this sense, is well shown by the dotted connections between the corresponding figures on either side of the diagonal. Also, it will be seen that the diagonal passes through the regions of greatest frequency. It follows that the diagonal in question represents the *locus* of average frequency. Now, along that diagonal, each value of A/C is associated with identically the same value of B/C; in other words, a correlation is found to have become established between them, which is solely due to the fact that *each* member in every couplet of A/C, B/C values is divided by the same value of the variable C.

We will now submit the above process to the test of extreme cases.

First; let the variability of A be so small that it may be treated as a constant, and take it = 1.

Then the values of A/C and B/C, that are severally associated with the three values of C, are as follows:—

Table III.

C.	A/C.	B/C.			Corresponding frequencies.
I	I	I	II	III	1 2 1
II	I/II	I/II	I	III/II	1 2 1
III	I/III	I/III	II/III	I	1 2 1

These frequencies are laid down at their proper places in fig. 2, where the three entries, corresponding to each successive value of A/C, run in vertical lines, but, on connecting the entries of maximum

frequency it is seen that they coincide with the diagonal from the O/O corner; also that the entries of minimum frequency are disposed symmetrically on either side of that diagonal and converge towards the same corner. Consequently, the existence of spurious correlation is manifest here. If B be the constant, and A and C the variables, the general results will of course be the same.

Secondly, let both A and B be constant and equal to I, and C the only variable; then there are only three possible combinations of A/C and B/C. In one of them both values are equal to I, in another to I/II, and in the third to I/III, all of which lie along the diagonal from (O, O), and thus testify to intimate correlation.

Lastly, let C be the only constant and equal to 1. Then A/C, B/C, become A and B, and the table of frequency of their various combinations is that shown in Table I and by the large figures in fig. 1, whose symmetrical disposition in all directions proves that there is no correlation.

“Report to the Committee of the Royal Society appointed to Investigate the Structure of a Coral Reef by Boring.”

By W. J. SOLLAS, D.Sc., F.R.S., Professor of Geology in the University of Dublin. Received December 31, 1896,
—Read February 11, 1897.

*Prefatory Note by Professor T. G. Bonney, D.Sc., LL.D., F.R.S.,
Vice-Chairman of the Committee.*

In presenting, as desired by the Committee, Professor Sollas's report on the attempts to ascertain, by boring, the structure of the atoll of Funafuti and on other investigations simultaneously undertaken, I avail myself of the opportunity of expressing the gratitude which is felt by its members to our friends in New South Wales, who have given such real and substantial help, especially by the loan of machinery and skilled workmen, in putting the project into execution; and among them chiefly to Professor Anderson Stuart (who has been practically another secretary in Australia), Professor Edgeworth David, Mr. W. H. J. Slee (Chief Inspector of Mines), and Sir Saul Samuel (the Agent-General of the Colony in England). I shall venture also to acknowledge gratefully the services of Captain Field and the officers of H.M.S. “Penguin,” and the unstinted labour which has been given by Mr. W. W. Watts, F.G.S., our Secretary in London, in carrying out our plans. In conclusion, may I express, speaking for myself, my earnest hope that another attempt will be made to determine the true structure of an atoll. I think, however, that our experience on this occasion shows that the attempt can be

much more easily made, and with a far greater probability of success, if Australia instead of England be the base of operations, and I trust that before long the colony of Sydney will initiate an expedition, and we shall co-operate with them as cordially as they have done with us.

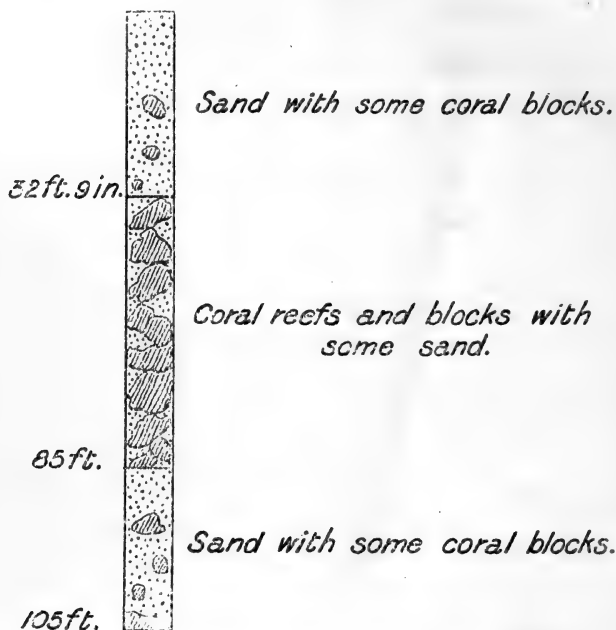
Report by Professor Sollas, D.Sc., LL.D., F.R.S.

H.M.S. "Penguin" having come to anchor in the lagoon of Funafuti on the afternoon of Thursday, the 21st of May, Captain Field at once landed with Lieutenant Dawson, Ayles (the foreman of the boring party), and myself, and we proceeded to make arrangements for our work on the island. A site for boring was chosen near the sandy beach of the lagoon, conveniently situated for the landing of gear, less than half a mile to the south and west of the village of Funafuti, and near the village well, which supplies a small amount of brackish but drinkable water. The work of landing was commenced the next morning, and completed by May 26. The erection of the boring apparatus was at once taken in hand, and on June 2, twelve days after our arrival on the island, all was in readiness for commencing operations. On June 3 the 6-inch tubes were driven into the sand, and by June 6 they had been advanced 30 feet; the 5-inch pipes were then entered and everything made ready for inserting the diamond crown and commencing to drill on Monday, June 8. On June 10 it was arranged that the work should proceed by shifts, so that the drilling might be carried on continuously day and night. During the first shift the crown had been advanced 20 feet, making the total depth then attained 52 feet 9 inches; during this shift fragments of highly cavernous coral rock were brought up in the core barrel from a depth of between 40 and 50 feet.

On June 11, a depth of 85 feet having been reached, it was found necessary to ream the hole preparatory to lining, and by June 15 the necessary reaming and lining had been completed. Up to this, although we had been somewhat disappointed at our slow rate of progress, occasioned partly by the unfavourable nature of the ground and partly by the frequent failure of our machinery, we had anticipated nothing worse than the possibility of finding our allotted time exhausted before we had reached a depth of 1000 feet; but now, on setting the crown to work, it very soon ceased to advance, and Ayles shortly afterwards came to me to announce that, in his opinion, the boring was a failure. Nevertheless, some further progress was subsequently made, and on Tuesday, June 16, a depth of 105 feet was attained. It then became once more necessary to ream and line the hole. Attempts to ream were continued all through Wednesday and Thursday but without success, sand poured into the hole and the reamer could not be driven through it. Efforts were made to remove

the sand by a sand-pump, but proved unavailing, the sand flowing in faster than it could be pumped out. Ayles assured me that it was impossible to descend another foot, and that he considered further labour as time and money thrown away. We decided therefore to abandon this borehole, and to recommence operations on another site, if possible in solid rock.

The structure of the ground passed through in the abandoned borehole was as follows:—



Although I knew of many places where solid rock forms the surface of the ground, it was very difficult to find one to which we could transport our machinery, the difficulties of landing on a rocky shore rendered several promising spots inaccessible by sea, while the absence of wheeled vehicles or even wheels, and the nature of the ground, seemed to put transportation by land out of the question.

At last, however, Mr. Hedley pointed out to me a portage called Luamanif, and used by the natives for dragging their canoes from the lagoon to the seaward side of the island, which at this place is very narrow, about 70 yards across. As this seemed a good landing-place, I submitted it to the consideration of Captain Field, who, after a personal examination, agreed that we might safely make use of it. Ayles and his party were then set to

work to sink trial pits on the line of the portage, one of these, situated 70 feet from the high-water mark on the seaward face of the reef, was sunk 12 feet through sand and blocks of coral, when operations were brought a close owing to the influx of sea-water at high tides. Two other pits were then commenced nearer the sea and a little to one side (north) of the portage, at the margin of the solid platform of rock, which extends down to the growing edge of the reef and which is covered by the sea at high-water. These passed through sand and fragments of coral. In the most northern of the two pits the sand was somewhat consolidated, and so, proceeding a few yards further north, as far in that direction as it would have been possible to transport our machinery, we opened another pit, which was sunk for a depth of 11 feet through fragments of coral, crystalline coral limestone, and partly consolidated sand. The bottom of the pit was 2 feet below the seaward margin of the reef, and as we were not inconvenienced by an influx of sea-water and Ayles was of opinion that the rock would "stand," we decided to make our new venture at this spot. Taking into consideration the difficulties of transporting our apparatus, I do not think a more favourable locality could have been chosen; it was close to the very edge of the rocky platform, which is so hard that Darwin, speaking of a similar platform in the case of another reef, says "I could with difficulty and only by the aid of a chisel procure chips of rock from its surface;" and as near the sea as it was prudent or even possible to go. Indeed, we had at first some doubt as to whether our pumping pipes would "live" in the surf of the ocean margin, and feared that the high-water spring tides might inundate the shaft; our fears in these respects, however, proved to be groundless.



Captain Field and myself were impressed with the need of additional boring apparatus, and he proposed that Ayles should go to Sydney to see if it could be procured. I gave much anxious consideration to this project, and discussed it with my colleagues, Messrs. Hedley and Gardiner, and with Ayles. The information I received from Ayles was not encouraging. He stated that we should require a complete equipment of lining tubes from 10 inches down to $2\frac{1}{2}$ inches in diameter, that 10-inch tubes were not to be had in Sydney, and that even if we succeeded in obtaining all the

appliances we required, the success of the boring would even then by no means be assured.

For a doubtful result I did not feel justified in incurring the certain increase in our expenditure which a journey to Sydney would have involved; the question of time had also to be considered, for had Ayles gone to Sydney we should on his return have been commencing our boring at or after the date the Committee had considered it would have been completed. Finally, it appeared that the new locality we had chosen for our work offered fair prospects of success.

The shaft already sunk to a depth of 11 feet was then timbered with Pandanus logs, and arrangements made for carrying down a hole by jumping with a 6-inch chisel. Ayles spoke of getting as far as 50 feet by this means, and then lining the hole with 6-inch tubes, but after sinking 4 feet he declared it impossible to proceed further in this way, the chisel could not be made to continue sinking in a straight line, the labour was too exhausting, and progress very slow. It was decided, therefore, to begin boring, Ayles being very hopeful, as the hole "stood" well. On Thursday, June 25, we accordingly made arrangements to shift our boring gear to the new site, and by Saturday, June 27, this work was completed, chiefly by native labour, and at a cost of about £10. The boilers were rolled along the beach, the rest of the machinery taken by water, and all subsequently dragged, rolled, or carried across the portage. Lieutenant Waugh lent us valuable assistance, during the absence of the "Penguin," in this work.

Boring was commenced on Friday, July 3, and by 5 o'clock we had sunk another 4 feet; progress then became rapid, and on Saturday evening, when work was knocked off, we had descended in all 46 feet. Very little "core" was obtained, however, and at times the boring bit met with very little opposition as it advanced, seemingly passing through a vacant space. Since the water pumped into the hole no longer flowed out above, but found its way out by some communication with the sea below, it was impossible to determine whether or not some sand might have been present. It was clear, however, that the coral rock through which the "bit" advanced was highly cavernous.

On Monday the hole became filled with fallen fragments and some sand, it was evident, therefore, that the sides would not hold, and so recourse was had to lining; by Thursday, July 9, the hole had been reamed and lined down to 45 feet, and the work of boring was resumed. On pumping, we had the satisfaction of seeing the water flowing out of the top of the hole, but our joy was short-lived, for, on Monday, June 13, the water was again lost. On Tuesday, July 14, we had reached 65 feet, passing for the last 20 feet through sand and coral. Subsequently we attained a depth of 72 feet, and could then proceed

no further. We worked all Thursday and Friday with the sand pump, but with no success; the bottom of the hole was surrounded by quicksand containing boulders of coral, and as fast as the sand was got out, so fast it flowed in and faster. The water pumped down disappeared through the sand, boring and *a fortiori* reaming was impossible, and the tubes could not be driven owing to the interspersed boulders. Had the tubes been provided with steel driving ends we might have forced them down; as it was, the effect of driving them was simply to curl in the lower end. Had we been provided with 4-inch tubes we could have made a fresh start, and might have descended another 30 or 40 feet, but even then ultimate success would not have been ensured, for the chance of meeting again and again with intermixed sand and coral remained always open, and every such encounter would have required lining tubes of diminished calibre.

Baffled in all our endeavours, and no other part of the island offering more hopeful prospects of success, we had no alternative but to abandon the undertaking, and on July 30 we were taken from the island in the "Penguin," and returned to Fiji. On landing there we had the mortification to learn that additional apparatus was then on the way to Funafuti, our friends in Sydney having with great generosity at once despatched machinery for driving in sand on receipt of a letter I had sent informing them of the failure of our first borehole. We had had no reason to expect such spontaneous assistance, and even had we been fortunate enough to have remained on the island till the machinery arrived, we should probably not have accomplished the object we had in view, though we might possibly have carried the borehole down to a depth of about 400 feet.

A very free communication must have existed between the borehole and the sea, for whenever a big roller broke upon the reef the rods lifted, and after the lining had been withdrawn, water spurted out of the borehole with the fall of every wave. The open nature of the reef is further indicated by the fact that the sea water rises with every tide to fill certain depressions, which occur in many places in the middle of the island; as the tide ebbs this water flows away down fissures, often so rapidly as to form little whirlpools.

Wherever I have seen the reef growing it has always presented itself as clumps or islets of coral and other organisms with interspersed patches of sand, and the borings would seem to indicate that it maintains this character for a very considerable depth and possibly throughout. The structure of the reef appears indeed to be that of a coarse "sponge" of coral with wide interstices, which may be either empty or filled with sand.

As regards the nature of this "sand," it is important to observe that it does not consist of coral *débris*; this material and fragments

of shells forming but an insignificant part of it; calcareous algæ are more abundant, but its chief constituents are large foraminifera, which seem to belong chiefly to two genera (*Orbitolites* and *Tinoporos*). It covers a considerable area of the islands, and has accumulated during the memory of the inhabitants to such an extent as to silt up certain parts of the lagoon. This and the abundant growth of corals and calcareous algæ, such as *Halimeda*, lead to the belief that the lagoon is slowly filling up.

A suggestion has recently been made that more light is likely to be thrown on the history of atolls by a study of ancient limestones in the British Isles than by boring in existing reefs. The first essential, however, for such a study would appear to be a knowledge of the structure of living atolls, for, without this, the identification of others forming a part of the earth's crust, might remain more or less a matter for conjecture. So far as the structure of Funafuti has been proved by borings, it is scarcely what a field geologist might have anticipated, and if deposits of a similar nature and origin should have been encountered in, say, the mountain limestone, it is doubtful whether, previous to the borings in Funafuti, their interpretation would have been easily reached.

While the boring has proved a failure, the other objects of the expedition have been attained with complete success. Messrs. Hedley and Gardiner have made a thorough investigation of the fauna and flora, both land and marine. Dr. Collingwood has obtained a good deal of information of ethnological interest, and we all have brought home a fairly complete collection of native implements and manufactures. A daily record was kept of maximum and minimum temperature, and of the readings of the dry and wet bulb thermometers.

The most important contribution, however, and one that I think must, in certain details, greatly modify our views as to the nature of coral reefs, is afforded by the investigations of Captain Field. Never before have soundings, both within and without an atoll, been so closely and systematically made, and the results seem to me commensurate with the care and pains that have been taken to secure them. Four series of soundings, "Sections" as they are termed on board the "Penguin," have been run from the seaward face of the reef outwards. How close together the soundings were made is shown in the following table which Captain Field has kindly permitted me to copy from his order book:—

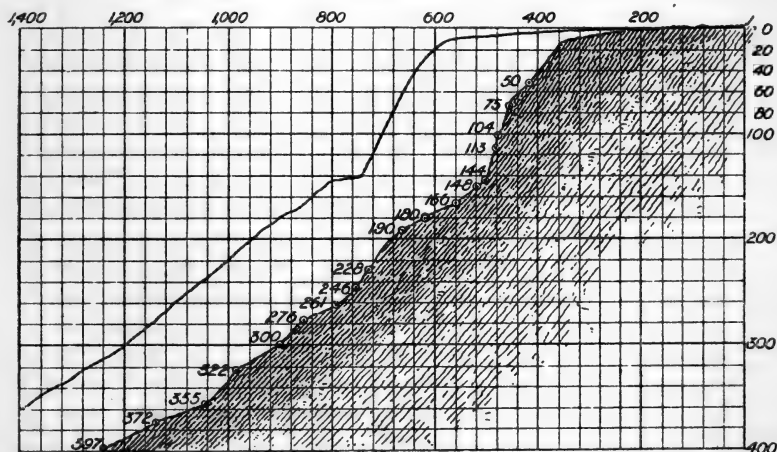
Depth	0— 40 fathoms every	10 yards.
„	40— 70	20 „
„	70—100	30 „
„	100—150	40 „
„	150—200	50 „

Depth 200—300 fathoms every 60 yards.

300—400	70	„
400—500	80	„
500—600	90	„
600—700	100	„
700—800	200	„

The profiles obtained by the four series are closely similar, and, as regards one important feature, almost identical. This is the sudden change in slope that occurs at or about 140 fathoms. Speaking generally, one may describe Funafuti as the summit of a submerged conical mountain, the base of which, at a depth of 2,000 fathoms, is a regular ellipse, 30 miles long by 28 miles broad. It rises with a very gentle slope, which gradually grows steeper as it ascends, till from 400 to 140 fathoms it has an angle of 30° ; at 140 fathoms an

Section D.



Two profiles of the ocean face of Funafuti. Vertical and horizontal scales identical. Figures on the vertical co-ordinate refer to fathoms, on the horizontal to yards.

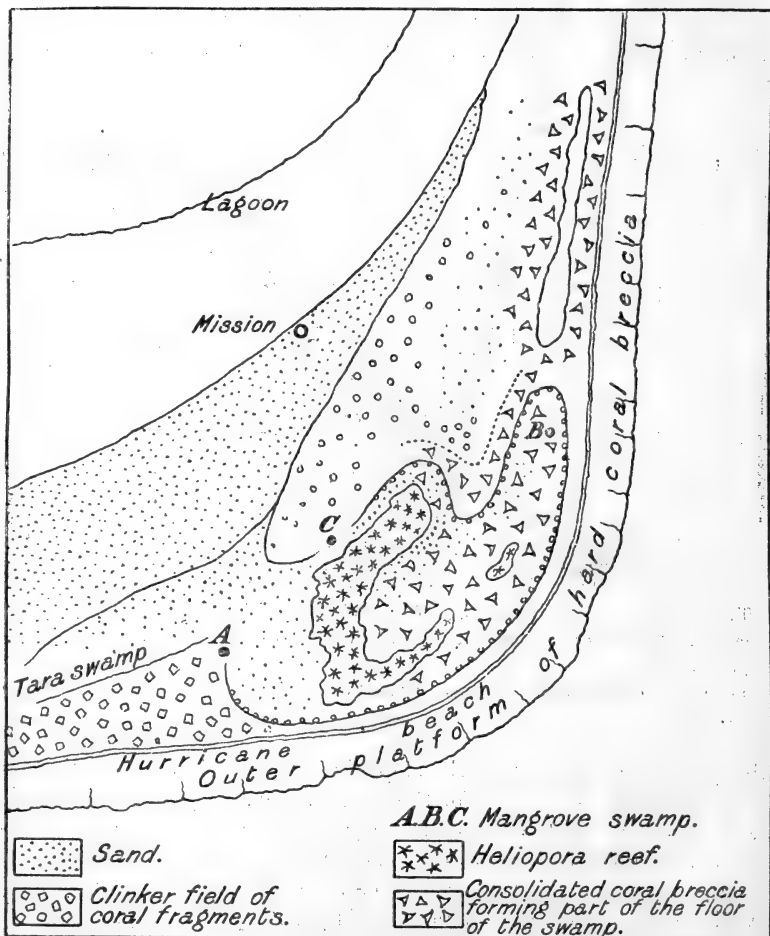
The curve on the left is supposed to commence 200 yards to the left of the zero point.

abrupt change occurs, and the slope becomes precipitous, making an angle of from 75° to 80° for the greater part of its course, till it passes into the shallow flats of the growing reef. It is difficult to resist the impression that it is the upper 140 fathoms (840 feet) which represents the true coral reef. A convex curvature of the profile between 166 and 261 fathoms is probably a talus, produced by an accumulation of coral *débris*.

The conical mountain below the 140 fathoms line, with its parabolic slope, is suggestively similar to a volcano; but, if so, its crater must have been immense, 10 miles across at least. A volcano, 12,000 feet in height, with a crater 10 miles in diameter, is, however, not an unknown phenomenon; within the limits of the Pacific we may cite Haleakala, in Maui, Sandwich Islands, as closely comparable.

A part of my work while on the island was the construction of a geological sketch map, part of which is shown below; its interest chiefly centres in a broad expanse near the Mission Station, where the two narrow limbs of the island meet, or, if it be preferred, whence

Corner of Funafuti, showing Mangrove Swamp and Heliopora Reef.



they extend. Towards the seaward side this broad corner is occupied by a mangrove swamp, the floor of which is formed by a dead coral reef, constituted almost wholly of two species, one a massive *Porites*, and the other *Heliopora cœrulea*. For a great part of the day this floor lies bare and dry, the frayed ends of the *Heliopora* standing like broken reeds, 6 inches above its surface, and the great clumps of *Porites* forming a series of stepping stones of equal height. Neither of these corals stands long exposure to the air; on Funafuti they require constant submergence, and we are thus led to regard their upper surface as marking what was at one time the level of low tide in the swamp; but since the present level of low tide is below the level thus indicated, some change must have occurred in the level of low tides. Not necessarily an elevation of the reef: Darwin has admirably discussed this explanation, and it is quite conceivable that some change in local conditions, such as the exclusion of the sea by the growth of the hurricane beach, may have produced a local alteration in the height of the tides. The swamp communicates with the sea by pits in its floor, which enter subterranean channels running seawards. These passages are so narrow that the tide rises and falls in the swamp much more slowly than in the open sea. To determine whether any change of level has taken place, it thus becomes necessary to compare the highest and lowest water level of the swamp with that of the sea or of the lagoon. I accordingly levelled across the island from the lagoon to the sea, crossing the swamp on the way, and found that the high-water level at spring tides is 1 foot 10 inches below high water (spring tides) of the lagoon, so that given free access of the sea, the *Heliopora* reef would be covered 1 foot 10 inches deeper than at present, but it is now submerged from 10 inches to 2 feet 2 inches at high-water springs, and would accordingly be submerged from 2 feet 8 inches to 4 feet, with free access of the sea. The range of spring tides is at least 6 feet, as I learn from Lieutenant Dawson, but I am not quite sure that an extreme range of 9 feet 8 inches has not been observed. Taking, however, the smaller number, it becomes clear that for a considerable part of the day, the reef would be exposed to the air. It is not likely that under these conditions the corals would continue to live, and I think, therefore, that the reef must have undergone some slight elevation, to the amount, perhaps, of 4 feet. This conclusion is in accordance with Dana's view, and is supported by observations on some other features of the island, such, for example, as the occurrence of an interrupted line of low cliffs, sometimes passing into a series of pinnacles, generally about 4 feet in height, as measured from low water level. In the annexed section the cliffs are farther from the land than is usually the case. These cliffs consist of a consolidated breccia of coral fragments, and are now in process of denuda-

SECTION THROUGH THE ISLET OF PAVA, FUNAFUTI.



Scale. Horizontal:— 4 fathoms to 0·1 inch. Vertical:— 6 feet to 0·1 inch.

tion, as is the coral platform which extends from them, up to and under the hurricane beach. This breccia was probably formed and cemented together when the reef stood at least 4 feet lower than at present, and was produced by the breakers driving fragments of corals from the seaward edge of the reef into the lagoon, as they are now doing over the isthmuses, submerged at high tide, which connect the several islets of the atoll together.

If it should prove true, as I do not doubt, that one of the latest episodes in the history of the reef has been an elevation of, say, 4 feet, then in the immediately antecedent stage, the reef must have been awash, or, perhaps, wholly submerged, and the present terrestrial fauna and flora must have reached it subsequent to its elevation, as sea drift, or have been introduced by human agency.

In conclusion, I would add that to myself the soundings obtained by Captain Field appear to support Darwin's theory of coral atolls; there remains, however, one very important branch of the subject which stands in need of renewed investigation, and this is the bathymetrical limit to coral life.

Not till I had obtained a close acquaintance with the difficulties of dredging on the steep sides of an atoll did I recognise on how frail a basis our accepted conclusions rest. It is a task difficult enough to get up corals from the lagoon in comparatively shallow water; from the sides of the reef it is well nigh impossible. To obtain dead corals from great depths proves little; living corals are generally found with dead associates, and the latter are the more readily detached and brought to the surface.

The weight of the evidence we already possess is admittedly in favour of a comparatively shallow bathymetrical limit, but much remains to be done before we can speak of any limit as definitely ascertained.

“The Influence of a Magnetic Field on Radiation Frequency.”
Communication from Professor OLIVER LODGE, F.R.S.
Received and read February 11, 1897.

I ask permission to bring before the notice of the Fellows a notable discovery recently made at Leyden by Dr. P. Zeeman, who is now elected Professor of Physics in the University of Amsterdam. To put myself in order, I will state that I have set up apparatus suitable for showing the effect, and have verified its primary feature, viz., that both lines in the ordinary spectrum of sodium are broadened when a magnetic field is concentrated upon the flame emitting the light.

Zeeman has observed it likewise with lithium, and with absorption as well as with emission spectra; taking precautions against deception by spurious effects due to changes of density or of temperature. It is thus probably not a chemical fact, dependent on the nature of a substance, but a physical fact, dependent on the nature of radiation and absorption, *i.e.*, a fact connected with the interchange of energy between ether and matter.

Faraday appears to have looked for some such phenomenon in the course of his latest magneto-optic researches in 1862, but he had not a Rowland concave grating at his disposal, and the effect is small.

I saw it with a 1-inch flat reflection grating containing 14,600 lines, and with an oxy-coal gas flame playing on pipe clay supporting carbonate of soda between pointed poles. I tried to see it by widening the slit till the D lines almost encroached on each other; thinking thereby to see the residual dark space obliterated by the magnetic action. A luminous haze seemed to spread over the dark chink when the magnet was excited, but the chink itself did not disappear; and the effect is more conspicuous and easier to observe when the narrowest slit possible is used, and when a micrometer spider-line is set down the middle of one of the D lines, of the second order spectrum, well defined in a field of considerable magnifying power.

The broadening is then unmistakable, and is symmetrical on each side; but I judge that the edges are not so bright as the central portion. The line appears brightened as well as broadened, *i.e.*, the previous borders of the line are brightened, and there are also gradated extensions. If the focussing is sharp enough to show a narrow, dark reversal line down the middle of either sodium line, that dark line completely disappears when the magnet is excited.

With the help of Professor H. A. Lorentz, the discoverer has initiated a simple theory of the effect, by considering the effect of

magnetic force on the motions of oscillating and revolving electrified particles possessing inertia (ions or electrons) in a magnetic field; and it is thus shown that the broadened edges of the line ought, on Lorentz's view, to be emitting polarised light, viz., plane polarised in directions normal to the lines of force, and circularly polarised in a direction along those lines.

This prediction has been experimentally verified by Zeeman, and has likewise been confirmed by myself. The flame being looked at from a direction perpendicular to the magnetic field, the light which will be dispersed by the grating to form the extended borders of a line is plane polarised, with its electric oscillations normal to the field's lines of force.

I hope to have the pleasure of communicating an English version of Professor Zeeman's complete paper to the March number of the 'Philosophical Magazine.'

"The Influence of a Magnetic Field on Radiation Frequency."

Communication from Dr. J. LARMOR, F.R.S. Received and read February 11, 1897.

In the course of the development of a dynamical hypothesis* I have been led to express the interaction between matter and ether as wholly arising from the permanent electrons associated with the matter; and reference was made to von Helmholtz (1893) and Lorentz (1895) as having followed up similar views. A footnote in Dr. Zeeman's paper has drawn my attention to an earlier memoir of Lorentz (1892), in which it was definitely laid down that the electric and optical influences of matter must be formulated by a modified Weberian theory, in which the moving electrons affect each other, not directly by action at a distance but mediately by transmission across the ether in accordance with the Faraday-Maxwell scheme of electric relations. The development of a physical scheme in which such action can be pictured as possible and real, not merely taken as an unavoidable assumption which must be accepted in spite of the paralogisms which it apparently involves,† was a main topic in the papers above mentioned.

The experiments of Dr. Zeeman verify deductions drawn by Lorentz from this view. It might, however, be argued that inasmuch as a magnetic field alters the index of refraction of circularly polarised light, which depends on the free periods of the material molecules, it must therefore, quite independently of special theory,

* 'Phil. Trans.,' 1894, A, pp. 719—822; 1895, A, pp. 695—743.

† H. A. Lorentz, "La Théorie Electromagnétique de Maxwell, et ses Applications aux Corps Mouvements," 'Archives Néerlandaises,' 1892. Cf. especially § 91.

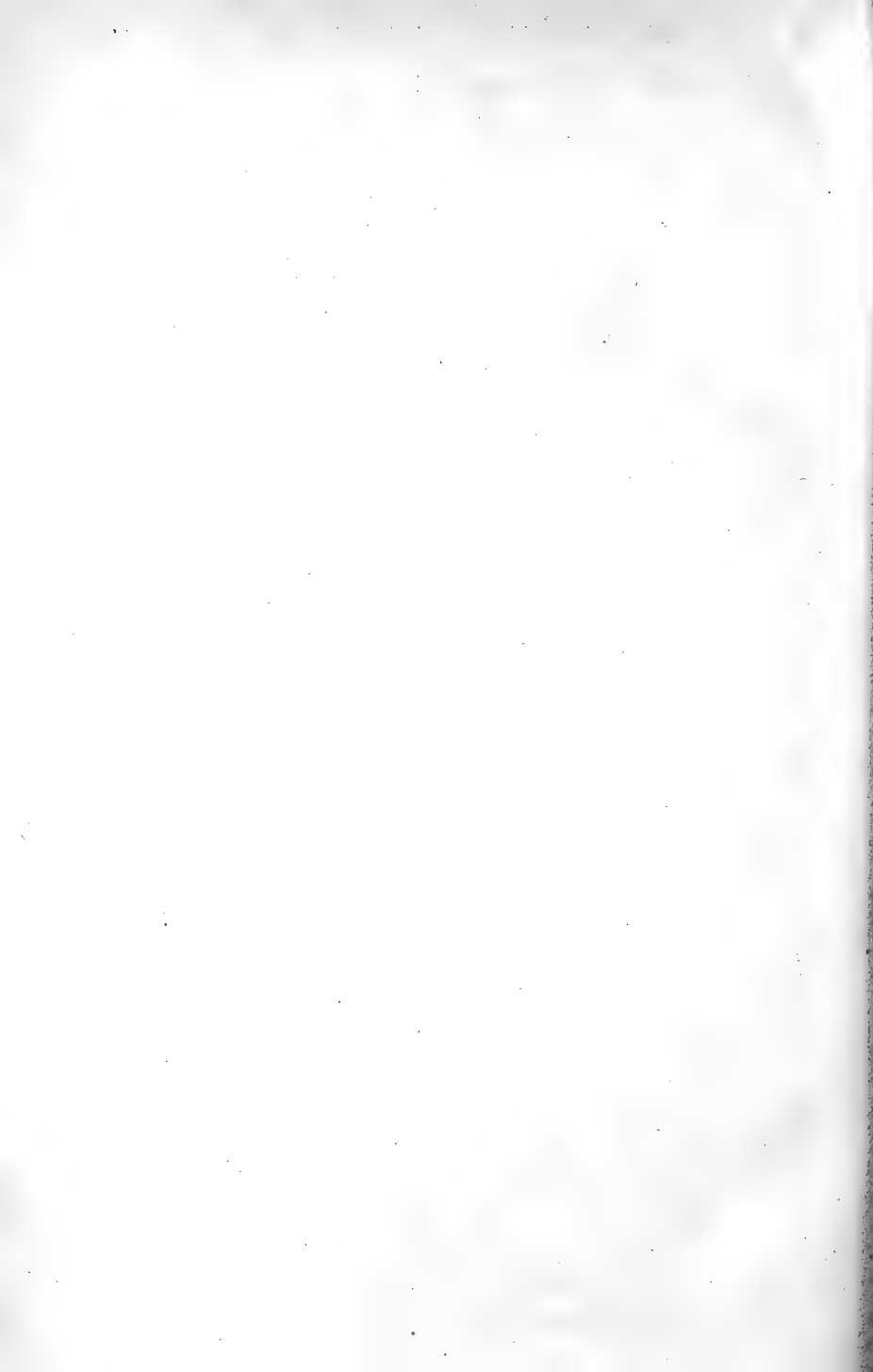
alter the free periods of the spectral lines of the substance. But the actual phenomena do not seem to be thus reciprocal. On the electric theory of light it is only the dispersion in material media that arises from direct influence of the free molecular periods, the main refraction arises from the static dielectric coefficient of the material, which is not connected with the periods of molecules.* From the phenomena of magneto-optic reflexion it may be shown that, on the hypothesis that the Faraday effect is due to regular accumulated influences of the individual molecules, it must be involved in the relation between the electric force (PQR) and the electric polarisation of the material ($f'g'h'$), of type

$$f' = \frac{K-1}{4\pi} P - c_3 \frac{dQ}{dt} + c_2 \frac{dR}{dt},$$

where ($c_1c_2c_3$) is proportional to the impressed magnetic field. This relation, interpreted in the view that the electric character of a molecule is determined by the orbits of its electrons, simply means that the capacity of electric polarisation of the molecule depends on its orientation with regard to the imposed magnetic field, that, in fact, the static value of K, depending on the molecular configurations just as much as do the free periods, is altered by the magnetic field. This relation agrees with the main feature of rotatory dispersion, namely, that it roughly follows the law of the inverse square of the wave-length. The specific influence of the molecular free periods, that is, of the ordinary dispersion of the material, on the Faraday effect, is presumably a secondary one; though it, too, follows the same law for different wave-lengths, in the case of substances for which Cauchy's dispersion formula holds good. It is this latter part of the Faraday effect that is reciprocal to Dr. Zeeman's phenomenon.

The question is fundamental how far we can proceed in physical theory on the basis that the material molecule is made up of revolving electrons and of nothing else. Certain negative optical experiments of Michelson almost require this view; at any rate, they have not been otherwise explained. It may be shown after the manner of 'Phil. Trans.,' 1894, A, p. 813 (and Dr. Zeeman's calculation, in fact, forms a sufficient indication of the order of magnitude of the result), that in an ideal simple molecule consisting of one positive and one negative electron revolving round each other, the inertia of the molecule would have to be considerably less than the chemical masses of ordinary molecules, in order to lead to an influence on the period, of the order observed by Dr. Zeeman. But then a line in the spectrum may be expected to arise rather from one of the numerous epicycles superposed on the main orbits of the various electrons in the molecule than from a main orbit itself.

* *Loc. cit.*, 'Phil. Trans.,' 1894, A, p. 820; and 1895, A, p. 713.



OBITUARY NOTICES OF FELLOWS DECEASED.

HERMANN KOPP, who was elected a Foreign Member of the Royal Society in 1888, and who died in Heidelberg on February 20, 1892, was born on October 30, 1817, at Hanau, where his father, Johann Heinrich Kopp, practised with some distinction as a physician. The father occupied himself in his leisure with experimental chemistry, and a few papers by him on mineral analysis and on physiological chemical products are to be found in Leonhard's 'Taschenbuch' and Gehlen's 'Journal.' The subject of this notice received his school training at the gymnasium of his native town, where he was well grounded in Latin and Greek. The facility he thus acquired in reading classical literature never left him, and proved of incalculable service to him in the preparation of his great work on the history of chemistry. At eighteen he went to Heidelberg, where he studied chemistry under Leopold Gmelin and physics under Wilhelm Muncke. At that time Heidelberg presented few opportunities for acquiring a knowledge of practical chemistry. Gmelin was Ordinary Professor of Medicine as well as of Chemistry, and his chemical teaching was regarded as subordinate to that of medicine. Kopp left Heidelberg for Marburg, where he graduated in 1838, presenting to the Philosophical Faculty as his thesis an essay entitled 'De oxydorum densitatis calculo reperiendæ modo,' in which we trace the germs of the experimental work by which he is best known. From Marburg he passed on to Giessen, attracted thither by the growing fame of the chemical laboratory which Liebig had called into existence. Here he made, under Liebig's direction, the only investigation in pure chemistry that he ever published, an unimportant paper on the decomposition of mercaptan by nitric acid, for the most part a repetition of the work of Löwig and Weidmann on ethylsulphonic acid and its salts.

Kopp, however, elected to cast in his lot with that of Giessen, and in 1841 he became *Privat Docent* in that University, lecturing on theoretical chemistry, crystallography, meteorology, and physical geography. He now began, when barely twenty-four years of age, his celebrated 'History of Chemistry,' the work by which he is best known to the literary world. In 1843 he became Extraordinary Professor, and on the departure of Liebig to Munich in 1852 he and Heinrich Will were made Ordinary Professors, and were placed in charge of

the Giessen laboratory. This position he resigned after the first year, leaving Will the sole control of the laboratory. Kopp remained at Giessen nearly a quarter of a century, and all his most important experimental work was done there. In 1863 he received a call from Heidelberg, which he accepted, and here he stayed until his death, occupying himself with lectures on the history of chemistry and on chemical crystallography. He was repeatedly solicited to accept a position in one of the larger Universities, notably in Leipsig and in Berlin, but all attempts to draw him from his dear Ruperto-Carolina were fruitless. "Even Bunsen alone," he was wont to say, "keeps me fast in Heidelberg."

Kopp's 'History of Chemistry' is his greatest literary effort. The first volume of it appeared in 1843, and the fourth and final volume in 1847. By the publication of this classical work, Kopp, when barely thirty years of age, suddenly found himself famous. His life-long friend, von Hofmann, who was then at Giessen, has left us the following account of the sensation which the work made on its appearance:—

"With one accord his contemporaries recognised that here was a production which, whether they regarded the thoroughness of research that it displayed, or the manner in which the material resulting from that research was sifted and arranged, was without a parallel in the literature of any other country. And even to-day, after the lapse of nearly half a century, there is no historical work on chemistry that can be even remotely compared with it. Numbers of books relating to the same subject, some of considerable merit, have since been published in Germany and France, but it is not difficult to perceive that they are all grounded on Kopp's great work."

For upwards of forty years Kopp had it in contemplation to bring out a new edition, and much of the later historical work he published, such as his 'Beiträge zur Geschichte der Chemie,' which appeared between 1869 and 1875, and the 'Entwicklung der Chemie in der neueren Zeit,' printed under the auspices of the Historical Commission of the Bavarian Academy in 1873, together with the two volumes on 'Die Alchemie in älterer und neuerer Zeit,' grew out of the materials he had gathered together. "But," again to quote Hofmann, "the better is here the enemy of the good. Kopp postponed the 'vermehrte und verbesserte Auflage' year after year, in the hope of being able to make a fuller study of certain special periods. Whoever is familiar with the mass of profoundly interesting matter he had accumulated, or who has had the opportunity of seeing the bulky note-books in which it was stored, must deeply lament that the hand which could alone arrange these treasures is now stiffened in death."

The literature of chemistry is further indebted to Kopp for the

part he played in the foundation and execution of the well-known 'Jahresbericht über die Fortschritte der Chemie und verwandter Theile anderer Wissenschaften.' This great work was, in a sense, the outcome and continuation of Berzelius' 'Yearbook.' On the death of the Swedish chemist in 1848, the leaders of the Giessen school of chemical thought determined to carry on his work of registering the progress of chemistry, but on a somewhat different plan. Berzelius at the time of his death was the greatest chemical critic of the time, and wielded his authority with all the despotism of an Oriental potentate. The 'Jahresbericht' of Liebig and Kopp differed fundamentally both in plan and execution from its Swedish prototype. It was to be a review of the year's progress, not only in chemistry, but also in all those sciences which were associated with chemistry, or were, in any definite sense, ancillary to it; it was to be done impartially, and with no special reference to any set of dogmas or particular school of chemical thought. Practically the whole of the more active members of the scientific side of the Philosophical Faculty of the University were concerned in its production. To Kopp fell the greater share of the arrangement, and of the general editorial management; in addition, he undertook the summaries relating to Theoretical, Physical, and Inorganic Chemistry. To Buff and Zaminer was entrusted Pure Physics; to Heinrich Will, Organic Chemistry; to Knapp, Technical Chemistry; to Ettling, Mineralogy; and to Dieffenbach, Chemical Geology. The first volume appeared towards the close of 1849, and consisted of a review of the work of 1847 and 1848. Liebig continued to be associated with Kopp as editor for some years after his removal to Munich, but in 1857 his place was taken by Will, who acted as co-editor until 1862, when Kopp resigned his share in the responsible direction of the publication just prior to his removal to Heidelberg. No chemist active in the prosecution of research needs to be reminded of the value of the 'Jahresbericht.' It has undoubtedly exercised a most beneficial influence on the development of chemical science in Germany, and it has been of the greatest service to those chemists in this country to whom German is not an unknown tongue.

In 1851 Kopp joined Liebig and Wöhler in the production of the 'Annalen der Chemie und Pharmacie,' and for many years he took the responsible share in its management. He prepared the section on "Theoretical Chemistry" in that well-known text-book, Graham-Otto's 'Lehrbuch der Chemie' and his 'Introduction to Crystallography,' written specially for chemists, was long a standard work.

Kopp's scientific papers relating to his experimental and critical labours appeared mainly in 'Poggendorff's Annalen,' and in the 'Annalen der Chemie und Pharmacie.' Two or three of his early communications were printed in the 'Philosophical Magazine,' and

his elaborate memoir, "On the Specific Heat of Compound Substances," in which he sought to develop Neumann's law, was published by the Royal Society. The 'Royal Society Catalogue of Scientific Papers' gives the number of his papers as 65.

Kopp enjoys an almost unique position as an investigator. The one consistent purpose of his work was to establish a connexion between the physical and chemical nature of substances; to prove, in fact, that all physical constants are to be regarded as functions of the chemical nature of molecules. It is not implied, of course, that the conception of such an interdependence originated with him. As a matter of fact, almost immediately after the publication of Dalton's 'New System of Chemical Philosophy,' in which the doctrine of atoms was revived to account for the fundamental facts of chemical union, the endeavour was made to connect the chemical attributes of a substance with one of its best defined physical constants, viz., its atomic mass. Prout's hypothesis is, in reality, the generalised expression of such an attempt; it is an adumbration of Mendeléeff's great discovery of the Law of Periodicity. But it may be justly claimed for Kopp that no one before him made any systematic effort to connect such of the physical qualities of substances as admit of quantitative statement with the stoichiometrical values of such bodies. The sporadic attempts made prior to 1840 were practically fruitless on account of the imperfect nature of the physical data up to that time extant.

When Kopp began his inquiries, very few boiling points were known, even approximately; and he had, as a preliminary step, to ascertain the conditions under which such observations must be made in order that accurate and comparable results could be obtained. The thermal expansions of barely half a dozen liquids had been measured, and the very methods of making such measurements with precision had to be worked out.

At the outset of his investigations, Kopp found the physical constants with which he was more immediately concerned very much as Berzelius found Dalton's values of the relative weights of the atoms; at the close of his work they were hardly less accurately known than were those stoichiometric numbers to the ascertainment of which the great Swedish chemist had dedicated his life.

Kopp's more important memoirs readily and naturally fall into comparatively few groups, viz., (1) those concerning the relations between the specific gravities of substances and their molecular weights; (2) those treating of the relations between boiling point and chemical composition; and (3) the papers relating to the specific heats of solids and liquids. As regards the other papers, only the briefest notice is here possible. Much of this work was of a pioneer character, and his conclusions have necessarily been modified by

subsequent research. His "law" of boiling points is no longer regarded as an accurate expression of experimental facts, and his deductions with respect to specific volumes have been largely affected by subsequent work. It has been conclusively shown that molecular volume is not a purely additive property. There is no longer room for doubt that the molecular volumes of substances are affected by far more conditions than Kopp was able to take cognisance of.

The value $\text{CH}_2 = 22$ has no other significance than as expressing the average increment in volume in successive members of a homologous series. Indeed, as the physical data increase, it becomes doubtful whether even this mean value is correct. Later observations appear to show that the value augments as the series is ascended. The relation $\text{C} = 2\text{H}$ no longer applies to carbon compounds in general. What is true of carbon and hydrogen is equally true of oxygen, whether as carbonyl- or as hydroxyl-oxygen. No definite or uniform values can be assigned to oxygen such that the molecular volume of a liquid can be *a priori* determined. The values given by Kopp are simply mean values, but the actual volumes are affected by conditions of which, as yet, we have no very precise knowledge or any certain means of measuring. The values for the other elements are, of course, affected by these considerations. Thus the specific volume of chlorine is obtained on the assumption that the values for carbon and hydrogen are constant. All, then, tends to show that the molecular volume is not the sum of constant atomic volumes.

Although Kopp's theoretical conclusions hardly admit of the generality which he assumed them to possess, his experimental work remains unassailed and unassailable, a monument to his ingenuity, manipulative skill, his rigid sense of accuracy, and illimitable patience.

T. E. T.

Dr. JOHN RAE, LL.D. (Edin.), a traveller in Arctic America, of extraordinary energy and endurance, a keen observer of Nature, and the discoverer of the fate of the Franklin expedition, was born in Orkney in 1813, died in London in 1893, and is buried in the cathedral of St. Magnus at Kirkwall, where a statue is erected to his memory.

He qualified as a surgeon in Edinburgh, and as such he accompanied one of the ships of the Hudson's Bay Company, whose service he joined, and then for ten years he resided at Moose Factory. (1) His first journey of pure exploration was a boat voyage along the coast of Hudson's Bay to Repulse Bay, where he wintered, and, in the following year he surveyed a coast line of 700 miles, connecting the surveys of Ross in Boothia with those of Parry at Fury and Heckla Strait. (2) Next he joined the expedition of Sir J. Richard

son in 1848 in search for Sir J. Franklin, during which the whole coast was explored that lay between the mouths of the Mackenzie and the Coppermine Rivers. (3) In 1851, at the request of Government, he explored and mapped, with the slenderest outfit, 700 miles of the south coast of Wollaston Land and Victoria Land, still in search of Sir J. Franklin, for which achievement he received the gold medal of the Geographical Society. Its result was greatly to narrow the range of possibilities as to the locality of the missing expedition. (4) He took charge of a boat expedition, proved the insular character of King William's Land, and came at last upon relics of Franklin's party and received verbal information from the Eskimo that gave the first definite information as to their fate. The disaster occurred at the mouth of the Back River, a little more than 200 miles in a direct line from the place where he heard of it. For this achievement he received the promised grant of £10,000 from Government. He did not visit the spot himself, but his information as to the site and the completeness of the disaster, was soon abundantly confirmed. After this he made some further travel of interest, though by no means of the importance of the above, surveying a route for a telegraph line across Iceland and in North America.

This bald statement of itineraries will give but a poor idea, except to Arctic travellers, of the severity of the work accomplished. To supply the deficiency, the following quotation is given from the address of Sir R. Murchison when presenting the Gold Medal to Dr. Rae; his remarks chiefly referring to the journeys numbered above as (1) and (3).

"With a boldness never surpassed, he (Dr. Rae) determined on wintering on the proverbially desolate shores of Repulse Bay, where, or in the immediate neighbourhood, one expedition of two ships had previously wholly perished, and two others were all but lost. There he maintained his party on deer shot principally by himself, and spent ten months of an Arctic winter in a hut of stones, the locality not even yielding drift timber. With no other fuel than a kind of hay made of the *Andromeda tetragona*, he preserved his men in health, and thus enabled them to execute their arduous surveying journeys of upwards of 1,000 miles round Committee Bay (the southern portion of Boothia Gulf) in the spring. Next season he brought his party back to the Hudson Bay posts in better working condition than when he set out, and with but a small diminution of the few bags of provisions he had taken with him.

"On his last journeys, in which he travelled more than 3,000 miles in snow-shoes, Dr. Rae has shown equal judgment and perseverance. Dreading, from his former experience, that the sea might be frozen, he determined on a spring journey over the ice, and performed a most extraordinary one. His last starting place at Fort Confidence on the

Great Bear Lake, being at a distance of more than 150 miles from the coast by the route he was compelled to take, he could not, as in the parties of our naval expeditions, travel on the ice with capacious sledges, and was, therefore, obliged to restrict his provisions and baggage to the smallest possible weight. With a pound of fat daily for fuel, and without the possibility of carrying a tent, he set out accompanied by two men only, and trusting solely for shelter to snow houses he taught his men to build, accomplished a distance of 1,060 miles in 39 days, or 27 miles per day including stoppages, and this without the aid of advanced depôts, and dragging a sledge himself great part of the way. The spring journey, and that which followed in the summer in boats, during which 1,700 miles were traversed in 80 days, have proved the continuity of Wollaston and Victoria lands along a distance of nearly 1,100 miles, and have shown that they are separated by a strait from N. Somerset and Boothia, through which the flood tide sets from the north. In this way Dr. Rae has performed most essential service, even in reference to the search after Franklin, by limiting the channels of outlet between the continent of America and the Arctic Islands."

It is easy to understand that Dr. Rae's views as to the equipment of expeditions in Arctic travel would differ in many respects, rightly or wrongly, from those who advocated the costly naval expeditions then in vogue. He could point to instances of his own superior success, and to the disasters that befel the survivors of the Franklin expedition, as they toiled homewards with a miscellaneous collection of heavy articles. Putting forward his views, as he did with point and insistence, his remarks were, as a rule, somewhat unwelcome to the naval authorities.

In early middle life Dr. Rae was remarkable for manly beauty in form and feature, combined with a temper that was quick and somewhat fiery. In a book on Ethnology, where each of the human races was represented by a single specimen, it was noticed that an old photograph of Dr. Rae had been utilised to represent the Caucasian type.

Dr. Rae's house contained an interesting series of specimens illustrating the fauna and flora of arctic America and the domestic methods of the Eskimo, which he delighted to show and to explain, for he was a most courteous host, well aided by his wife. As a narrator he was delightful, being always lucid while full and circumstantial. His memoirs and speeches were stamped throughout with those characteristics.

His interest in the regions where he gained his fame remained unabated to the last. He died, regretted by many friends, in his eightieth year.

FRANZ ERNST NEUMANN was born on September 11, 1798, at Joachimsthal, a small town about forty miles to the north-east of Berlin. At the early age of seventeen he entered the army as a volunteer to fight against Napoleon in the campaign of 1815. A serious wound, received in the battle of Ligny, kept him to his bed for many weeks; but, on recovery, he once more joined the army. At the end of the war he returned to his lessons at the "Gymnasium" of Berlin, and subsequently entered the University as a student of theology. Soon afterwards he migrated to Jena, where he came under the influence of C. S. Weiss, the Professor of Mineralogy, and turned his attention to that subject. His papers, published between 1823 and 1830, all referred to crystallography, and even his earliest work attracted attention, and left a lasting impression on the science of mineralogy. It secured him a call to the University of Königsberg as "Privat-docent," where Bessel, Jacobi, and Dove became his colleagues. Under their influence he gradually drifted more and more towards the study of physics. His knowledge of mathematics was acquired by private study, for although the University of Berlin nominally possessed a teacher of mathematics, no lectures were given. If the circumstances of Neumann's early education are considered, it is remarkable that he obtained such a command of mathematical physics, and this seems to have been ascribed by himself to the careful study of Fourier's writings, which he admired to such an extent that he made a manuscript copy of the great treatise on the 'Conduction of Heat.' In the year 1828 Neumann was appointed Professor Extraordinarius at a salary of 200 thalers (£30). Bessel, who had formed a high opinion of his powers, wrote in the same year a letter to the Minister of Education pressing Neumann's claim to a better position. The letter had the desired effect, and Neumann was nominated, in 1829, Professor Ordinarius, and his salary raised to £75. He never left Königsberg, continuing his professorial duties until 1876, and died on May 23, 1895.

Among his earlier papers on physical subjects, attention must be drawn to one on the specific heat of minerals (*Pogg. Ann.*, 1831). It contains an extension of Dulong and Petit's law of specific heats to compound bodies having a similar chemical constitution, but is chiefly valuable for the improvement, both in the methods employed and in the theoretical discussion of the experimental results. It is shown how the method of mixture may be applied to the case of badly conducting substances. The second paper treats of the specific heat of water. The older observers had stated that when hot water is poured into cold water, the resulting temperature of the mixture is lower than that calculated, on the assumption that the specific heat of water is constant. Neumann showed that this result is due to errors of experimentation, and demonstrated with

improved apparatus, that the specific heat of water increases with rising temperature. On the assumption that the rate of change is uniform, Neumann calculated the ratio of the specific heats at 100° and 0° to be 1.0176. The assumption made is now known to be incorrect, but it cannot be said that Neumann's experimental result has been much improved upon by later investigators. Although nearly all fields of physical science have at different times been successfully treated by Neumann, his fame chiefly rests on his theoretical investigations in optics and electricity. After Fresnel's fundamental researches, which had shown the possibility of explaining the most complicated optical phenomena by the undulatory theory, it became necessary to connect that theory more closely with the conditions of wave-propagation in ordinary elastic bodies. In other words, an elastic solid theory of the ether formed the next step to be taken, and the name of Neumann will always remain associated together with that of Cauchy, McCullagh, and Green in the early efforts to found a truly dynamical theory of light. In the first paper, "Theorie der doppelten Strahlenbrechung abgeleitet aus den Gleichungen der Mechanik," Neumann obtains a wave-surface identical with that deduced somewhat earlier by Cauchy. In the case of biaxial crystals it does not agree with that of Fresnel. It consists of three sheets, one of them being due to the longitudinal wave. The difference of the two other sheets with Fresnel's surface is, however, more nominal than real, for as Stokes pointed out, in his Report on Double Refraction, the difference may, by proper adjustment of the constants, be made to show itself only in the tenth place of decimals. The same report gives full details on the comparison between the theories of Cauchy, Neumann and Green. A further important contribution to optics was made in the year 1835 under the title "Theoretische Untersuchungen der Gesetze, nach welchen das Licht an der Grenze zweier vollkommen durchsichtigen Medien reflectirt und gebrochen wird." This paper raises the difficult question of the mathematical expression for the conditions which must hold at the surface separating two crystalline media. For well considered reasons Neumann adopts the view that the density of the ether is the same in all media, and follows out this hypothesis to its logical consequences. The same problem was treated at the same time by McCullagh by very different and simpler methods, but the results of both investigators were identical. Neumann further confirmed his equations by experiment. The general acceptance of the electromagnetic theory has now considerably changed our point of view, but the historical importance of Neumann's work must be conceded in spite of certain defects which may, with justice, be urged against it.

Several further papers treated of optical subjects, amongst which,

perhaps, the most important refers to double refraction in strained uncrystalline bodies.

Neumann* next turned his attention to electricity, and in two important papers, published in 1845 and 1847, established the laws of induction of electrical currents. We meet here, for the first time, with the "electrodynamic potential." It is shown how currents, induced in one circuit either by the motion of conductors carrying electric currents, or by a change in the intensity of the current, may be deduced from one function depending on the relative position of the conductors, and that this function will also determine the mechanical forces acting between the conductors. To appreciate fully the great advance which was made by these two memoirs, it is necessary to realise that the papers were published before it had been shown, by Helmholtz and Lord Kelvin, how the principle of the conservation of energy may be utilised in the treatment of the problem. It may also be pointed out that Neumann's investigations are deduced from Lenz' laws, which are direct consequences of the principle of energy; so that Neumann's treatment may, indirectly, be said to depend on that principle.

Neumann was the first to solve the problem of the magnetisation induced in an ellipsoid of revolution under the action of any magnetic forces. Other important contributions relate to the functions known as spherical harmonics. It is a matter for regret that his first paper on that subject ('*Astronomische Nachrichten*,' 1838) was completely overlooked by magneticians until Ad. Schmidt recently drew attention to it. The method which might with great advantage have been employed in the treatment of terrestrial magnetism, may be explained by reference to the simpler problem of expanding a function of one variable by means of Fourier's series. For instance, if the daily changes of temperature are to be expressed in such a series from hourly readings of the thermometers, a very simple and well-known process leads to the determination of the constants. Neumann's investigations led him to an analogous process for the expansion of a function in a series of spherical harmonics, the functions having known values at the points of intersection of certain latitude and longitude circles on a sphere.†

Neumann's last publication was a memoir (edited by his son, C. Neumann), '*Beiträge zur Theorie der Kugelfunctionen*,' which contains many interesting theoretical researches on that subject.

* Neumann's initials are often incorrectly given; thus, in the text of Maxwell's '*Electricity and Magnetism*' (second edition) he is uniformly quoted as J. Neumann.

† In both the problems mentioned the values of the constants are really indeterminate, but the solution gives, under certain assumptions, their most probable values. Care should be taken that in any actual problem the assumptions are really justified.

Neumann's publications are not sufficient to give an adequate idea of his life's work. As a teacher he exerted a wide-spread influence, and the progress of physical science in Germany is largely indebted to the stimulating influence which he exercised, especially with the help of the 'Mathematisch-Physikalisches Seminar,' founded by him in conjunction with Jacobi and Sohnke. The object of this institution was to supplement the teaching given in lectures, and to introduce students into the methods of original research. Exercises were set to the students by the directors of the seminar, and, as Neumann himself explained, "In the choice of problems I laid stress on their referring to points of practical importance, such as the application of Gauss' theory of principal points and planes in a system of lenses; or that the selected exercise should lead students to an experimental investigation of a problem which they had treated in a theoretical manner."

There was never, probably, a school of original research conducted in so systematic a manner as this seminar, in which Neumann was the leading spirit. Annual reports of the work done by each student were sent in to the Prussian Minister of Education, and, occasionally, money prizes were given for a research of special merit. An interesting account of the history of this seminar is contained in a notice of Neumann's life by P. Volkmann.* Its importance may be recognised by the fact that Kirchhoff's first papers on the distribution of electric conductors, and H. Wild's construction of his photometer and polarimeter, figure amongst the direct results of the teaching given in the seminar. Kirchhoff's great powers were soon recognised by Neumann, and when, in the year 1846, Neumann had set as a special prize problem "The determination of the constants on which the intensity of induced currents depends," the prize was awarded to him for a research which contained the first measurement of a resistance in electro-magnetic measure. Neumann's success as a teacher will be appreciated by reference, in Volkmann's publication, to the doctor dissertations of his pupils, which were carried out under his guidance. Amongst the students who flocked to hear his lectures at Königsberg, we find Borchardt, Durège, Lipschitz, Kirchhoff, Wild, C. Neumann, Clebsch, Auwers, Quincke, and Voigt.

Neumann was elected a Foreign Member of the Royal Society in 1862, a Corresponding Member of the French Academy in 1863, and received the Copley Medal in the year 1887.

A. S.

* Leipzig (G. Teubner), 1896. I owe to this publication and to Mr. Voigt's notice in 'Göttingen, Nachrichten,' 1895, p. 248, nearly all the information given in the above obituary notice.

By the death of Sir JOSEPH PRESTWICH British geological science loses one of its oldest, as well as one of its most distinguished votaries. Descended from an old Lancashire family (in which, for some cause or other, a baronetcy has lain dormant for some generations), he was born at Pensbury, Clapham, on March 12, 1812.* After some preliminary schooling he was sent to Paris, where he remained for two years in a school attached to the Collège Bourbon. He was then transferred to Dr. Valpy's, at Reading, and finally entered University College, London, soon after its establishment. He there worked diligently in the chemical and natural philosophy classes under Dr. Turner and Dr. Lardner, availing himself also of the geological and mineralogical collections in the British Museum.

While still at College he started a Society among his fellow students, each member of which had in his turn to deliver a lecture on chemistry or some branch of natural philosophy. This "Zetetical Society" had rooms of its own, and a small laboratory, in Surrey Street, Strand. It consisted of about fourteen members; but its existence was of limited duration. Mr. Prestwich himself was called away from it to join the business of his father, who was a well-known wine merchant in Mark Lane; and he remained closely connected with the house and business for nearly forty years. Happily, his commercial avocations to some degree aided, instead of restricting, his pursuit of geological studies. He had to make frequent visits to France and Belgium, in both of which countries he formed lasting friendships with the leading geologists and palæontologists of the day; and he made himself personally familiar with the actual strata and fossils which they had described. Not only so, but his business among the country connexions of the firm carried him to nearly every part of the United Kingdom, and the hours unclaimed by his engagements were enthusiastically devoted to the study of the local geology of the districts he visited. His comprehensive eye enabled him rapidly to appreciate and to grasp the leading features, topographical and geological, of most of the areas which in those days possessed an exceptional geological interest; and those who in later years had the good fortune to accompany him to such spots were surprised to find how retentive was his memory and how intimate was his acquaintance with every pit, quarry, and rock-section that in any way illustrated the geological problem under consideration.

His first published papers dealt with the fossil-bearing deposits of the neighbourhood of Gamrie, Banffshire—particularly with the strata containing ichthyolites, and with the shell-bearing layers of the Till—and the international character of his geological work was exhibited

* For much that is here said I am indebted to a memoir by Dr. Henry Woodward, F.R.S., published in the 'Geological Magazine,' 1893, p. 242. I have also to thank Professor Lapworth for kind assistance.

by his following paper, on "Les Débris de Mammifères terrestres qui se trouvent dans l'Argile plastique aux Environs d'Épernay." Though written at an earlier date, these memoirs were not published until 1837. He had already, in 1833, become a Fellow of the Geological Society. His memoir on the "Geology of Coalbrookdale," published in the Transactions of that Society in 1836, was founded mainly on visits made to Coalbrookdale in the years 1831 and 1832. This work, which was accompanied by descriptions of new plants and mollusca by his friend Professor Morris, was the earliest monograph on the structure of a British coalfield. It at once established his reputation as a geologist, and it has ever since been numbered among our British classics.

From about 1846 onwards for several years, his attention was mainly concentrated upon the tertiary deposits of the London basin, and he published a work on the water-bearing characters of these deposits in 1851. But the scientific results of his investigations were of far higher importance. He not only reduced the little known English tertiaries into proper system (establishing the separate existence of certain local beds to which he gave the name of the Thanet Sands, proving the synchronism of the Reading beds with those of Woolwich, and fixing the true position of the London clay with respect to the Hampshire basin), but he succeeded in correlating the tertiary beds of England, France, and Belgium in such a manner that his classification was accepted by most geologists, and has stood the test of time.

This comprehensive study of the tertiary group naturally led Mr. Prestwich onward to the investigation of the later and more superficial deposits; and the acquaintance which the writer of these pages had the good fortune to form with him in 1851, led to an enduring friendship and constant intercourse, as well as to occasional geological excursions with him to spots where these drift and alluvial deposits could be examined. In the winter of 1858, Dr. Hugh Falconer urged upon Mr. Prestwich's attention the desirability of investigating in the field the evidences for the discoveries of M. Boucher de Perthes of flint implements of prehistoric man in the gravel deposits of the Valley of the Somme, which were then somewhat doubtfully received, and in April, 1859, Mr. Prestwich proceeded to Abbeville, where he was joined by Mr. John Evans. Thence they went to Amiens, and in the gravel beds of St. Acheul saw for themselves, still embedded in its matrix, one of those implements of unquestionable human workmanship, the asserted existence of which in the alluvial deposits had met with so much doubt. The previous discoveries, thus verified and subsequently supplemented by researches conducted on lines which could with confidence be laid down, soon led to an entire revolution in the then existing ideas as to the antiquity of man. Not that the new views were at

once accepted, or that the advocates of the old ideas were backward in their defence of them. For years controversy was long and occasionally loud; but so completely has it now died out, that the promoters of what were then new views occasionally find themselves at the present time in antagonism with the promoters of views newer still, for which they are not quite satisfied that there is as yet sufficient foundation.

At various intervals, from 1859 onwards, Mr. Prestwich wrote several papers relating to post-Pliocene deposits, including one of great importance, "On the Loess of the Valleys of the South of England and of the Somme and of the Seine," communicated to the Royal Society in 1862. He had previously furnished to the Society an account of the discoveries of flint implements at Abbeville, Amiens, and Hoxne.

In 1866 and 1867 Mr. Prestwich rendered valuable aid to the country by acting on the Royal Coal Commission, and on that on the Metropolitan Water Supply. In connection with the former he furnished an exhaustive, and at the same time suggestive, Report (published in 1871) "On the Probability of finding Coal under the Newer Formations of the South of England"—some of the anticipations in which he lived to see at all events partially realised.

With regard to the latter subject, his book, 'The Water-bearing Strata of the Country around London,' gave evidence of his capacity to speak.

During all these years Mr. Prestwich had been actively engaged in business, and it is amazing to note the amount of detailed and accurate geological work that he was able to accomplish. But about 1872 he managed to emancipate himself in a great measure from the trammels of trade, and in 1874 he was appointed to succeed the late Professor Phillips in the Chair of Geology at Oxford. He was there able to devote nearly the whole of his time to the prosecution of his favourite study, and to enlisting recruits for the science.

It is impossible in a notice of this kind to cite even the titles of his numerous papers, but especial mention may be made of his memoirs "On the Temperature of the Sea at various Depths below the Surface," and "On the Origin of the Parallel Roads of Lochaber" (printed in the 'Philosophical Transactions'), as well as those on "Underground Temperature" and on the evidences of the "Submergence of Western Europe."

To the Institution of Civil Engineers he communicated essays on the "Geological Conditions affecting the Construction of a Tunnel between England and France," and on the "Origin of the Chesil Beach," for which a Telford Medal was awarded him.

His papers read before the Geological Society were numerous. Among his later ones, those on "Volcanic Action," on the "Mundesley

and Westleton Beds," on the "Relation of the Glacial Period to the Antiquity of Man," on the "Pre-glacial Drifts of the South of England," and on the "Age of the Valley of the Darent," may, perhaps, be described as the more important.

It was while living at Oxford that he produced, in 1886 and 1887, his great work in two volumes on "Geology, Chemical and Physical, Stratigraphical and Palæontological." In this work he not only brought forward many arguments against carrying the doctrine of uniformity too far in attempting to read the history of the earth, but at the same time he showed some signs of reverting to theories involving more of cataclysmic action than most modern geologists are willing to allow. As a whole, however, his book is a monument of patient and conscientious work, and is likely long to retain the high position that it holds at present in geological literature.

As already stated, Mr. Prestwich was elected a Fellow of the Geological Society so long ago as 1833. From 1856 onwards he for many years served the Society as Treasurer, becoming President for two years, from 1870 to 1872. Already in 1849 the Wollaston Medal had been awarded him for his researches at Coalbrookdale and in the London Basin.

In 1853 he was elected a Fellow of the Royal Society, and at intervals served upon its Council, during seven years in the aggregate. In 1870—1871 he was a Vice-President of the Society. One of the Royal Medals was awarded to him in 1865 for his contributions to geological science.

In France the name of Prestwich was almost as well known as in England. He was one of the oldest members of the French Geological Society, and when it was assembled at Boulogne, in 1880, he was appointed President of the meeting. In 1885 he was elected a Corresponding Member of the Institut (Académie des Sciences). He was also a Foreign Member of the Accademia dei Lincei, at Rome, of the Geological Institute of Vienna, and of various academies in Belgium, Switzerland, and the United States of America. When the International Geological Congress met at London in 1888, the esteem with which he was regarded by geologists of all nationalities was shown by his unanimous election as President of the Congress.

He retired from the Geological Chair at Oxford in 1888, to the great regret of his brother professors, and of his numerous friends in that University, which conferred upon him in the same year, as a tribute of esteem, the honorary degree of D.C.L. After his retirement he resided for the most part at his delightful country house, Darent Hulme, Shoreham, Kent, which he built, in accordance with his own tastes some twenty-seven years ago, and every room and wall of which brought to mind some subject of geological interest, either in material or decoration. There he actively continued his scientific

labours, efficiently aided and cared for by a loving wife—the niece of his old friend, Dr. Hugh Falconer.

The first public recognition of his services, both to science and the State, was accorded him at the beginning of the present year, when he received the honour of knighthood, with the unanimous acclaim of the scientific world. But he was, alas! not destined to bear his honours long, and, after some months of great physical weakness, he died on June 23rd, 1896.

Of his personal amiability, his devoted friendship, and his charm of manner, this is hardly the place to speak: but all those with whom he was brought into contact will agree that in Sir Joseph Prestwich we have lost not only one of the great pillars of geological science, but a geologist whose mind was as fully stored with accumulated knowledge as that of any of his contemporaries, and one who was always ready to place those stores generously and freely at the disposal of others.

J. E.

GEORGE JOHNSON was born in November, 1818, at Goudhurst, in Kent, and he received his education at the Grammar School there. In 1837 he paid a visit of some weeks to an uncle who was a medical practitioner in Cranbrook, and became so enamoured with the life of a country doctor that he decided to join his uncle as an apprentice. There he remained for two years and a half, and then entered the medical department of King's College, London, with which institution his name has been so intimately connected ever since. His college life was a highly distinguished one; he obtained numerous prizes and scholarships both at the College and at the University of London, where he took his degree of M.D. in 1844. At King's College Hospital he served as clinical clerk to Dr. Todd, and dresser to Sir William Ferguson; later on he became house physician, house surgeon, and, in 1843, resident medical tutor. At the end of his college course he was elected an Associate of King's College.

This brilliant academical career altered his intention of becoming a country practitioner, and he decided to remain in London. In 1846 he became a Member of the Royal College of Physicians, and four years later was elected a Fellow. At the College of Physicians he filled many important offices, including those of Examiner in Medicine, Councillor, Censor, Vice-President, Goulstonian Lecturer, Lumleian Lecturer, and Harveian Orator. In 1862 he was appointed a Senator of the University of London; in 1872 he became a Fellow of the Royal Society; and, in 1884, President of the Royal Medical and Chirurgical Society.

His appointments at King's College Hospital were those of Assistant Physician (1847), Full Physician (1856), Professor of

Materia Medica (1857); and Professor of Medicine, in succession to Dr. George Budd (1863). In 1886 he resigned this post, and was elected by the Council, Emeritus Professor of Clinical Medicine, and Consulting Physician to the Hospital. Shortly after this he became a Member of the Council of King's College, in which position he continued to serve his *alma mater* until his death.

In 1883, Dr. Johnson was appointed by the Prince of Wales Consulting Physician to the Royal College of Music; in 1885 he received the honour of being elected a member of the Athenæum Club, on the ground of his eminence in science; in 1888 his past and present students and friends presented him with his portrait, painted by the late Mr. Frank Holl, R.A. This picture was publicly presented to him in the large theatre of King's College amid a crowd of his former colleagues and friends by Sir Joseph Lister. The scene will long be remembered by all those who heard Sir Joseph Lister's kindly words, and Dr. Johnson's emotional reply. In 1889 he was made Physician Extraordinary to the Queen, and in 1892 he received the honour of knighthood.

The following list comprises his principal contributions to literature:—"On Diseases of the Kidney, their Pathology, Diagnosis, and Treatment" (1852); "Lectures on Bright's Disease" (1873); "Epidemic Diarrhœa and Cholera" (1855); "Notes on Cholera" (1856); "The Laryngoscope" (1864); "A Defence of Harvey as the Discoverer of the Circulation of the Blood" (1884); this was a reply to certain criticisms evoked by his Harveian oration of 1882. In 1887 he published a collection of medical essays and lectures in which many of his former ideas were stated with new force. Sir George Johnson's scientific life was by no means a peaceful one, and led to much controversy; he continued to take part in discussions arising from his work until the very last. In 1894, in a series of letters to the 'Lancet,' he maintained, in opposition to Dr. Pavy, that normal urine contains no sugar, but that the principal reducing substance present is creatinine, a material which he and his son (Mr. G. S. Johnson) very thoroughly investigated. In 1889 he published an essay on "Asphyxia," in which he defended his well-known views against those of his opponents. As late as 1895, a 'History of the Cholera Controversy,' in which Sir George played so prominent a part, appeared from his pen; and in the present year a similar book on 'The Pathology of the Contracted Granular Kidney' completed his long series of publications.

He married, in 1850, Charlotte Elizabeth, youngest daughter of the late Lieutenant White, of Addington. He was left a widower with five children ten years later.

The vigour of Sir George Johnson's mind remained unimpaired to the last, but his bodily health was feeble. He suffered from paralysis

agitans, was subject to insomnia, and was slightly deaf. These infirmities rendered his attendance at public meetings somewhat irregular, but when questions of urgency arose he was always at his post at the Senate of the London University, the Council of King's College, and the meetings of the College of Physicians. During the last three or four years, however, his health had improved, and he was able during his summer holidays to resume his shooting in Scotland, a sport of which he was extremely fond. Only last summer he related with pride how he had brought down a stag at the distance of so many yards. His house in Saville Row contained many trophies of the chase. His sudden end on Wednesday, June 3, 1896, therefore came as a surprise and shock to all his friends. The cause of death was apoplexy. The morning of Monday, June 1, he was in his usual health, and he employed it in writing a paper which was published in the 'Lancet' of June 13, under the appropriate title, "A Last Word on Cholera." This was a brief criticism on Dr. Kenneth Macleod's article on "Cholera," in Dr. Clifford Allbutt's 'System of Medicine.' In the afternoon he went out for his usual drive, and it was on his return that he was seized with hemiplegia. Though he regained sufficient consciousness to recognise those about him, he never rallied, and died within forty-eight hours of the attack.

The funeral took place on June 8, after a preliminary service at St. James's, Piccadilly, conducted by Dr. Wace, Principal of King's College, and attended by a large number of his friends and admirers, Sir Joseph Lister representing the Royal Society; the remains were laid to rest by the side of those of his wife at St. Mary's, Addington.

The medical and scientific world has lost a distinguished ornament, an earnest and steady worker, a deep thinker, a vigorous writer, and a lovable and tender-hearted friend.

The foregoing enumeration of the principal incidents in his life shows how full it was of active service, but cannot paint the man as he was to those who knew him. The readers of his works will see in him the trenchant writer, and the uncompromising but always fair defender of his views. Those who listened to his lectures will remember the well ordered, logical, and clear exposition of his thoughts; here he never allowed his strong but contentious ideas to appear in undue relief when he was teaching his students. His opponents will know him as a hard hitter, but one who was always ready to acknowledge his own mistakes, and who never carried his words into the region of personal attack. It is, however, only those who sat with him by his fireside who can properly realise the generous friend, the lovable disposition, the keen interest he always took in questions of science, and the enthusiasm with which he followed up his theories. It was especially the younger men with whom he

liked thus to show his sympathy, and among his scientific friends he used to say that above all he dearly loved to chat with the physiologists.

It is somewhat difficult for one like the present writer, who only knew Sir George during the last ten years or so of his life, to guess who among his earlier friends had most to do with the formation of his character. Sir George had obviously a strong character of his own, which would have brought him to the front in any walk of life; but to judge by his conversation on the reminiscences of his younger days, it would seem that above all others, Dr. Todd was the one who especially stimulated him in the particular branches he took up. At the time that he was student, Dr. Todd was Professor of Physiology at King's College, and throughout the whole of his subsequent life, Johnson was as diligent a student of physiology as he was of medicine. He knew, in a most surprising way, the contents of modern physiological text-books, especially in relation to the circulation of the blood, his favourite study; and, to show the vigour of his mind, he was intensely interested towards the last in the question of osmotic pressure, a difficult subject which has only recently attained importance to physiologists. He was, however, not merely a student of books, but was practical to the backbone; after the establishment of the physiological laboratory at King's College, during the time Professor Rutherford occupied the chair of physiology, he was a frequent visitor there, and much important work was done at his suggestion then and subsequently. He was an accomplished histologist, and took a keen delight in showing to his friends the specimens by which he believed he had refuted the views of those who disagreed with him. Even in the last week of his life he had commenced experiments on the action of the cilia in the renal tubules of the newt's kidney.

In mentioning his early friends, one must not omit to enumerate Sir Thomas Watson, whom he helped with his celebrated lectures; Sir William Ferguson, Sir William Bowman, and Dr. Bristowe, all of whom Sir George Johnson survived.

The controversies of his life were numerous; there were stormy times at King's College, especially in years now far back; there was the great cholera controversy: in the first years of this, Johnson was most unfairly treated, being branded almost as a quack in the medical journals. He, however, in spite of loss of practice, stuck to his views, and had, in the end of his days, the satisfaction of seeing his evacuant treatment of cholera regarded as a rational one, and in many cases recognised by eminent practitioners as the correct one. Of his *sobriquets*, Johnson preferred to be known as "Cholera Johnson" rather than "Kidney Johnson." His views on the kidney question were direct deductions from physiological knowledge derived

from the discovery of the muscular structure of the arterioles by Henle, and the work of Claude Bernard on vasomotor nerves. His views on the cause of the hypertrophied heart in cases of Bright's disease are now generally regarded as correct. His ideas on asphyxia, which he continued to the last to call by the old-fashioned, but etymologically correct, name, apnoea, formed the subject of another spirited debate; and, in conclusion, one must mention a controversy of another kind, the dispute with Sir William Gull over a point of professional etiquette connected with the "Balham Case." The point was decided in Johnson's favour by the College of Physicians, but the incident left a good deal of bitterness behind it.

Still this long series of struggles did not embitter Johnson's life. He was always able to discuss the matters involved without a trace of ill-feeling, though a mention of any one of them would lead him into a prolonged and forcible exposition of his own views.

In his later essays he was able to write with calmness, and was willing to leave to time the recognition of what was true in the active and full life-work, which he must have known was then drawing to a close.

W. D. H.

HENRY NEWELL MARTIN was born on July 1, 1848, at Newry, County Down, Ireland. He was the eldest of a family of twelve, his father being at the time a Congregational minister, but afterwards becoming a schoolmaster. Both his parents were Irish, his father coming from South Ireland, and his mother from North Ireland. He received his early education chiefly at home; for though he went to several schools, his stay was not long at any one of them.

Having matriculated at the University of London before he was fully sixteen years of age (an exemption as to age being made in his favour), he became an apprentice to Dr. McDonagh, in the Hampstead Road, London, in the neighbourhood of University College, on the understanding that the performance of the services which might be required of him as apprentice, should not prevent his attending the teaching at the Medical School of the College, and the practice at the hospital. During his career at University College he greatly distinguished himself, taking several medals and prizes, in spite of his time for study being, on account of the above-mentioned duties, less than that of his fellow students. In 1870 he obtained a scholarship at Christ's College, Cambridge; he had, in the summer of that year, conducted at Cambridge a class of Histology for the late Sir G. Humphry. The writer of this notice had about the same time been appointed Prælector of Physiology at Trinity College, and the two went up to Cambridge together in the October of that year. He at once undertook to act as the demonstrator of the

Trinity Prælector, whose right hand he continued to be in every way during the whole of his stay at Cambridge. His energy and talents, and especially his personal qualities, did much to advance and render popular the then growing School of Natural Science in the University. At that time there was, perhaps, a tendency on the part of the undergraduate to depreciate natural and, especially, biological science, and to regard it as something not quite academical. Martin, by his bright ways, won among his fellows sympathy for his line of study, and showed them, by entering into all their pursuits (he became for instance, President of the Union and Captain of the Volunteers) that the natural science student was in no respects inferior to the others.

In Cambridge, as in London, his career was distinguished. He gained the first place in the Natural Science Tripos of 1873, the second place being taken by Francis M. Balfour; at that time the position in the Tripos was determined by the aggregate of marks in all the subjects. While at Cambridge he took the B.Sc. and M.B. London, gaining in the former the scholarship in Zoology; he proceeded later to the D.Sc., being the first to take that degree in Physiology. So soon as, or even before, he had taken his degree, he began to devote some time to research, though that time, owing to the necessity under which he lay of making money by teaching, was limited; his first publication was a little paper of the structure of the olfactory membrane, which appeared in the 'Journal of Anatomy and Physiology' for 1873.

In the summer of 1874 he assisted the Trinity Prælector in introducing into Cambridge the course of Elementary Biology, which the late Professor Huxley had initiated at the Royal College of Science during the preceding year. He subsequently acted as assistant in the same course to Professor Huxley himself. One result of this was that he prepared, under Huxley's supervision, a text-book of the course which, under their names, appeared with the title 'Practical Biology,' and which has since been so largely used.

In 1874 he was made Fellow of his College, and giving himself up with enthusiasm to the development of natural and, especially, of biologic science at the University, was looking forward to a scientific career in England, if not at Cambridge. About that time, however, the Johns Hopkins University at Baltimore was being established, and such was the impression made by Martin upon those with whom he came in contact, among others Dr. Gilman, of Baltimore, that in 1876 he was invited to become the first occupant of the Chair of Biology which had been founded in the Johns Hopkins University. This offer he accepted, and thus nearly the whole of his scientific career was passed in America. He went out prepared to develop in his new home the higher teaching of biologic science, especially that

spirit of research which alone makes teaching "high"; and during the rather less than a score of years which made up his stay at Baltimore, he produced a very marked effect on American science, fully working out the great aim of the University which had adopted him. By himself, or in concert with his pupils, he carried on many important investigations, among which may especially be mentioned those on the excised mammalian heart. He was the first to show that by appropriate methods the excised mammalian heart may be made the subject of prolonged study. One of these researches, namely, that on the "Influence of Temperature," was made the Croonian Lecture of 1883. His various contributions were, in 1895, republished in a collected form by his friends and pupils in America, under the title of "Physiological Papers." He sent out into the States, from among his students, a number of trained physiologists, fired with his own enthusiasm, who are continuing to advance the science, and one of whom has succeeded him at Baltimore. He also found time to write expository works, and his 'Human Body,' 'Briefer Course,' and 'Elementary Course,' deservedly became very popular in the States.

Upon his first appointment he had the charge of the whole subject of animal biology; and since he was himself more distinctly a physiologist, it was almost his first duty to secure or train up a colleague who should devote himself to morphology. Martin early saw the worth of one of his students, W. K. Brooks; to him he gradually entrusted morphological matters, and thus prepared, not only the way for a separate Chair of Zoology, but also the man to fill it.

Martin married in 1879 Mrs. Pegram, the widow of an officer in the Confederate army; but there was no issue, and in 1892 his wife died.

Even before his wife's death his health had begun to give way; and after that event he became so increasingly unfitted for the duties which his own previous exertions had raised to a very great importance, that in 1893 he resigned his post.

After his resignation he returned to this country, for he had never become an American citizen, and was looking forward to being able, with improved health, to labour in physiological investigations, either at his old University or elsewhere in England. But it was not to be. Though he seemed at times to be improving, he had more than one severe attack of illness, and never gained sufficient strength to set really to work. During the past summer he visibly failed, and while he was striving to recover his strength by a stay in the quiet dales of Yorkshire, a sudden hæmorrhage carried him off on October 27, at Burley-in-Wharfedale, Yorkshire.

Having been for so long a stranger to this country, Martin was,

personally, but little known in English scientific circles; in America, however, not in Baltimore only, but in many other parts of the States, especially among the younger physiologists, he has left behind him a memory which will not soon pass away; while those in this country who knew the brightness of his early days will always hold him in affectionate remembrance.

M. F.

BRIAN HOUGHTON HODGSON, of the Bengal Civil Service, oriental scholar, zoologist, and diplomatist, was born in February, 1800, at Prestbury, Cheshire, and was the eldest son of B. Hodgson, Esq., of Lower Beech, in that county. He belonged to a long-lived family; his father attaining his ninety-second year, and a grandmother and a great-grandmother their ninetieth. He was educated at Dr. Davies' school, Macclesfield, and was, according to the wishes of his great uncle the Bishop of London, and relative the Dean of Carlisle, intended for the Church; but, having no desire for holy orders, at sixteen years old a nomination to the East India College of Haileybury was obtained for him. Pending the passing his preliminary examination at Haileybury, young Hodgson was the guest of Professor Malthus, then preparing the seventh edition of his "Principles of Population," who directed his attention to politics as a career; whilst a casual presentation at the Governor's house to Canning, then President of the Board of Control, who addressed the youth with a brilliant sketch of the career possible to an Indian civilian, fired him with ambition to become a diplomatist, of which his stirring career, at the Court of Nepal, was the fruit. At Haileybury, Hodgson gained high honours in languages and political economy, finally passing out in 1817 as "First of his year." In 1818 he sailed for Calcutta, where he passed a year in the College at Fort William, studying the vernacular, Sanskrit, and Persian, and becoming a proficient in the latter. At Calcutta his health broke down, and, after a severe attack of fever, no choice was left him between abandoning the service or obtaining a hill appointment. The latter—an all but unattainable prize for an untried youth—was, nevertheless, thanks to his early promise, and more to the private influence of powerful friends with the Government, obtained for him, and he was appointed Assistant to the Commissioner of Kumaon, a province of the Western Himalaya ceded by the Nepalese a few years previously.

Fortunately for Hodgson, his chief, G. W. Traill, was a first-rate official, and, equally fortunately, Kumaon was, in a condition of disorganisation and savagery that taxed the highest qualities of its new rulers. It was Traill's first duty to obtain the confidence of a people driven into the jungles of all but pathless mountains by the

alternating tyrannies of Affghans and Ghurkas, and who recognised but two classes of beings—themselves and their ghosts; then to introduce the rudiments of justice, and, finally, raise the condition of the people to that of a prosperous British province. It was during his two years' pupilage with Traill that Hodgson commenced his zoological observations and those studies of the aboriginal tribes of India and their languages, which he pursued throughout his career; and, so efficiently did he perform his official duties, that, after two years (in 1820), he found himself unexpectedly promoted to be Assistant to the British Resident at the Court of Nepal. Here, however, a disappointment awaited him. He found the Resident, the Honourable E. Gardner, giving effect to Lord Hastings' wise policy of converting Nepal from a turbulent neighbour into a quiescent, if not friendly, ally of the British power, and this he was doing so effectively that Hodgson found a truce established, and no scope for his ambition as a politician and diplomatist. He accordingly applied to Government for more active employment, and was at once gazetted to the Secretariat of the Persian Department of the Foreign Office, Calcutta, a step towards the highest positions in the service. At Calcutta his health, as before, at once broke down, and he was fortunate in being sent again (in 1824) to Nepal in a subordinate position, awaiting the successorship to the Assistant Residentsip, which post had been filled up. This he obtained in the following year, followed by that of Acting Resident on Mr. Gardner's retirement (1829), and Resident in 1833.

It was during the enforced quiescence of Hodgson's first years in Nepal that he undertook the systematic study of Nepalese and Tibetan Buddhist literature, and the collection and description of the vertebrata of the Himalaya. By his courteous treatment of the Lamas of the temples of Katmandu and of the emissaries of the Grand Lama of Lhasa to the Nepal Court, he enlisted their active co-operation in the purchase of MSS., and in procuring copies of others, some dating back to upwards of 1100 years before the Christian era, for which latter purpose he kept a staff of copyists in constant employ. So impressed was the Buddhist hierarchy by his learning and labours, and so great was his reputation, that the Grand Lama of Lhasa himself sent him a copy of their classical scriptures, the *Kaghyur* and *Stangyur*, in 347 folio volumes. Subsequently Hodgson procured another copy which he sent to the college at Fort William, and which is now in the library of the Bengal Asiatic Society. Altogether, dating from 1824, he had given upwards of 270 volumes of Sanskrit and Tibetan literature to British institutions, especially to the Indian Government, and 147 to the *Société Asiatique de Paris*. The receipt of the latter in France, together with copies of his own researches in Buddhism, were, as early as 1837, recognised by the bestowal on him

of the Foreign Fellowship of the above Société, accompanied by the award of a gold medal, inscribed "Au fondateur de la véritable Étude du Budhisme par les textes et les monuments." This was followed, in 1838, by the Cross of the Legion of Honour, and, in 1844, by his election as a Correspondent of the Institute of France. Meanwhile his contributions to his own Government lay unheeded in the cellars of the old India House in Leadenhall Street; and there they remained till their transference to the present India Office, where the Kaghyur and Stangyur* occupy an apartment to themselves, accessible to all.

Scarcely less valuable and as extensive were Hodgson's contribution to zoology, especially ornithology, which rival his Buddhistical attainments. Throughout his residence in the Himalaya he was himself an assiduous collector, besides keeping a staff of shooters who penetrated even into Tibet, and of stuffers and artists at the Residency. He described systematically and minutely almost every species which he procured, accompanying the descriptions with anatomical details, and observations on their habits, nidification (if of birds), and geographical distribution. He published 127 zoological papers, chiefly in the 'Journal of the Asiatic Society of Bengal.' In 1843 and 1858 he placed 9512 specimens of Himalayan birds, 9037 of mammals, and 84 of reptiles at the disposal of the British Museum, together with 1853 drawings. Of the above the duplicates were distributed to the chief museums of Europe and America.

Very early in his career, Hodgson commenced a study of the Non-Aryan Races of India, their origin, customs, their unwritten languages, which he reduced to writing, their religions and geographical distribution. The results are embodied in twenty-seven papers contributed (with one exception) to the 'Journal of the Asiatic Society of Bengal.' These, in the opinion of Latham and other scholars, are of the highest value and rank as his chief services to literature.

Mr. Hodgson was a zealous advocate of the employment of the vernacular for instruction in the primary schools of India. In this his great opponents were Macaulay, Sir L. Trevelyan, and H. H. Wilson, who advocated English or a classical Oriental tongue. In 1835 he published two letters on the state of Education in India, which first "lifted the subject out of the arena of public controversy." For twenty years he persisted in his efforts, which were not crowned with success till 1854, when the Court of Directors adopted his views, which were further confirmed by the Education Commission of 1882.

But diplomacy was Hodgson's earliest and abiding ambition, and

* For a very imperfect copy of these works the Russian Government lately paid £2000.

for the exercise of this he had ample scope during the ten years of his residence at the Nepal Court. The latter, never friendly to the British alliance, was distracted by the often murderous intrigues of Raja, princes, queens, ministers, and a dominant military class of aggressive disposition, and Hodgson's main efforts were directed to the establishment of trading relations with Nepal, and to warding off or rendering abortive measures that would have led to hostilities with the Company's forces, especially during the crises of the Chinese, Affghan, and Punjab Wars. He persistently advocated the policy of enlisting the fighting class of Nepal in the British Army as a safe outlet for its activity, and it was greatly due to his influence with his friend Jung Bahadur, and his representations to Lord Canning, then Governor-General, that the former placed a Ghurka force at our disposal during the Mutiny.

In 1843 Mr. Hodgson retired from the service, and after a year's visit to England, and disposing of his later collections, he returned to India with the intention of pursuing chiefly his ethnological studies. For this object he took up his residence at Darjiling, a recently created health resort, nearly 7500 ft. above the sea, in the unexplored Himalaya, east of Nepal. Here he resided for sixteen years, in indifferent health, the result of repeated fevers in Nepal, but as indefatigable as ever in collecting and publishing in continuation of his Buddhist, zoological, and ethnological work, and in furtherance of the adoption of vernacular education.

In 1858 he finally returned to England, and resided first at the Rangers, Dursley, in Gloucestershire, whence he removed in 1867 to the Grange, Alderley, in the same county, frequently visiting London during the summer months. Latterly, the winters were passed at the Villa Himalaya, Mentone. He married first, in 1863, Miss Anne Scott, daughter of General H. A. Scott, R.A.; and, in 1868, Susan, daughter of the Rev. Chambre Townshend, of Derry, Cork, who survives him. He was elected a Fellow of the Linnean Society in 1835, and of the Royal in 1877; Corresponding Member of the Royal Asiatic Society in 1828, and Vice-President in 1876; Correspondent of the Zoological Society in 1859; D.C.L. (Oxon.) in 1889; and Fellow or Correspondent of many other scientific and literary bodies. The honours so early showered on him by France are given above. In person Mr. Hodgson was very good-looking, and of singularly frank and courteous bearing, communicative, and generous to a fault. His was a remarkable case not only of inherited longevity, but of complete recovery in after life from the effects of long-continued and often serious indisposition in India. He was fond of out-of-door exercise, and hunted till disabled by accident at sixty-eight. He retained his faculties but little impaired till his death in the summer of 1894, leaving no family. He was buried at Alderley.

WILLIAM CRAWFORD WILLIAMSON was born at Scarborough, on November 24, 1816. His father, John Williamson, who began life as a gardener, was a man of considerable scientific attainments, and was, for twenty-seven years, curator of the Scarborough Museum. From him his son early acquired a practical knowledge of geology and natural history. Williamson, in his recently published autobiography,* describes how, when a boy, his evenings, throughout a long winter, were devoted to naming fossil specimens from the neighbouring coast, with the aid of Phillips' 'Geology of Yorkshire.' "Pursuing," he says, "this uncongenial labour, gave me in my thirteenth year a thorough practical familiarity with the palæontological treasures of Eastern Yorkshire. This early acquisition happily moulded the entire course of my future life."

Williamson in those early days came into contact with several distinguished men of science, and, notably, with William Smith, the father of English geology, who spent two years in the Williamsons' house.

A little later, in 1832, he made the acquaintance of Murchison, who was already a friend of his father's, and from whom the younger Williamson received great kindness.

Williamson early adopted the medical profession, and during his apprenticeship to a Scarborough apothecary, found time to carry on his work in natural history, spending his holidays in shooting rare birds, and collecting plants and fossils. He wrote a paper on rare Yorkshire birds, when only about 16, and almost immediately afterwards he made his first contributions to fossil botany, drawing and describing many of the specimens for Lindley and Hutton's 'Fossil Flora of Great Britain.' More than thirty of the plates in this well-known book bear his name.

A paper on the distribution of organic remains in the Lias series of Yorkshire was read before the Geological Society of London, on May 9, 1834, when the author had only attained the age of 17½, and another in November, 1836, on the Oolitic fossils of the same coast. These were remarkable contributions to science in themselves, and the more so as coming from so young a worker; few naturalists can have started serious investigation so early in life.

Before he was 18, Williamson appeared as an author on a very different subject, for, in 1834, he published an account of the excavation of a tumulus at Gristhorpe, near Scarborough. This, which was probably his only archæological publication, was important in its effect on his scientific career, inasmuch as it brought the young naturalist into communication with the distinguished geologist, Dr. Buckland. Through his influence, this paper was reproduced in the

* 'Reminiscences of a Yorkshire Naturalist,' by W. C. Williamson, Redway, 1896.

Literary Gazette.' In a letter to Williamson, referring to this, Dr. Buckland said, "I am happy to have been instrumental in bringing before the public a name to which I look forward as likely to figure in the annals of British science." "The letter of Dr. Buckland," says Williamson, "was one of those influences the effect of which was unmitigatedly healthy."*

In 1835 Williamson was appointed curator of the museum of the Natural History Society at Manchester, an office which he held for three years while pursuing his medical studies. Several papers, chiefly on geological subjects, were the fruit of this period. In 1840 Williamson left Manchester and came up to London, where he entered as a student at University College. He here attended the lectures of the botanist Lindley, who now for the first time made the personal acquaintance of his young coadjutor.

While in London he was offered the post of naturalist to an expedition up the Niger, an offer which, fortunately for him and for science, he declined, for the undertaking ended disastrously.

After about a year's work in London, Williamson passed his qualifying examinations at the Apothecaries' Hall and College of Surgeons, and then returned to Manchester, where he at once commenced the practice of medicine. At first he found it necessary to keep his scientific pursuits somewhat in the background, but this did not last long. His interest in Ehrenberg's discovery of the Foraminifera in chalk led him to undertake microscopic research, a field of inquiry on which he had not previously entered. His first histological investigation, in 1842, related to the development of bone, a subject to which he returned a few years later. In the meantime he engaged seriously in the study of Foraminifera, following up Ehrenberg's work above referred to. Among the naturalists who supplied him with material for this investigation was Charles Darwin, then just returned from his famous voyage in the "Beagle." The results of Williamson's studies were embodied in a paper published in the 'Transactions of the Literary and Philosophical Society of Manchester' for 1845, on "Some Microscopical Objects found in the Mud of the Levant and other Deposits, with Remarks on the mode of Formation of Calcareous and Infusorial Siliceous Rocks." This was the most important of his works up to that date, and helped to lay the foundation of our knowledge of the part played by Foraminifera in the formation of geological deposits.

Williamson continued the study of these minute organisms, confirming the conclusions of Dujardin as to their affinities, and demonstrating the great variability of the living species. Many years later, in 1857, he completed his monograph for the Ray Society on the

* 'Reminiscences of a Yorkshire Naturalist,' page 47.

recent Foraminifera of Great Britain, after publishing a number of shorter memoirs on the group.

In 1851-2 Williamson made a careful study of the organisation of *Volvox Globator*, and brought out facts as to the mode of connection between its cells, which have only been verified by other observers within the last few years. This was probably his best contribution to recent botany.

Shortly before this date Williamson had undertaken an investigation of a totally different kind, namely, the development of the teeth and bones of fishes, which he considered in relation to the cell theory. His results in this field were of great importance, and are embodied in two papers published in the 'Philosophical Transactions of the Royal Society' for 1849 and 1851. The value of these investigations was recognised by his election as a Fellow of the Royal Society in 1854.

Previously to this, in January, 1851, Williamson had entered the ranks of official teachers of Science, by his appointment as Professor of Natural History in the newly founded Owens College at Manchester. This was an arduous post, for the subjects to be taught included three sciences: zoology, botany, and geology. At first he found it possible to deal with this formidable task, by spreading his complete course over two years, a wise arrangement under the circumstances, but one which the exigencies of the examination system ultimately rendered impracticable. This led in 1872 to a division of the duties of the chair, Williamson being then relieved of the geological part of the teaching by Professor Boyd Dawkins. The remaining work, however, was still far too extensive for any one teacher, and in 1880 a further division of labour took place. The late Professor Milnes Marshall occupied the chair of zoology, while Williamson retained that of botany, which he continued to hold till 1892.

In addition to his strictly official work as Professor, Williamson was one of the first two members of the staff, who, as early as 1854, started evening classes for working men. In later years, he met with extraordinary success as a popular scientific lecturer, more especially for the Gilchrist Trustees, for whom he delivered some hundreds of lectures during the period from 1874 to 1890. His power of rousing and retaining the interest of great popular audiences is described by those who have heard him as most remarkable.

During a great part of the time at Owens College, Williamson continued in active and successful practice as a physician. In the midst of all his multifarious duties, as professor, popular lecturer, and medical practitioner, he always found time for original scientific work; rarely has so busy a man done so much for the advancement of science by actual research.

So far, little has been said of the work of Williamson on fossil

botany, the subject with which his name is now most intimately associated, as it occupied all the latter part of his career as an investigator. His interest in such matters goes back, as has been mentioned above, to the very beginning of his scientific life. In addition to his work for Lindley and Hutton, a paper of his on the origin of coal was read before the British Association as early as 1842. His first original contribution to fossil botany dates from the year 1851, when he published a paper "On the Structure and Affinities of the Plants hitherto known as *Sternbergiæ*," in which he demonstrated their true nature as casts of the pith-cavity of Gymnospermous trees. A few years later, in 1854-5, he published papers on what was then called *Zamia gigas*, an extraordinary oolitic fossil, which Williamson believed to have Cycadean affinities, a view which has since been so far confirmed that the fossil is now regarded as representing the fructification of one the *Bennettitæ*, an allied, though very different family. Williamson's full memoir on the subject was written soon after 1855, but, owing to a succession of misfortunes, its appearance was long delayed, and it only saw the light in the 'Linnean Society's Transactions' for 1868, when it was published simultaneously with Mr. Carruthers' well-known paper on fossil Cycadean stems. The latter author founded a new genus for *Zamia gigas* under the name of *Williamsonia*.

Williamson's really characteristic work in fossil botany consisted in the investigation of the histological structure of carboniferous plants. The first beginning was made with the paper on *Sternbergia*, but it was not till long afterwards that the long series of publications began, which have done more than the works of any other writer to make us acquainted with the organisation of Palæozoic plants. It was early in the fifties that Williamson made his first sections, but not till 1868 that, in consequence of a correspondence with the French palæobotanist, Grand'Eury, he published the result of his investigations in the paper "On the Structure of the Woody Zone of an undescribed Form of Calamite," 'Manchester Literary and Philosophical Society's Proceedings,' Ser. 3, vol. 4. From that period onwards, his whole time available for original research was devoted to the Carboniferous Flora, and a magnificent series of memoirs was the result, which will always rank among the classics of fossil botany. The Royal Society alone published in the 'Philosophical Transactions' nineteen memoirs from his hand, their dates ranging from 1871 to 1893, and, besides these, many valuable papers appeared elsewhere, notably the memoir on *Stigmaria ficoides*, published in 1886, by the Palæontographical Society. It is impossible here to attempt anything like a summary of this great work, which threw light on every department of Palæozoic botany.*

* For fuller information see Williamson's 'Reminiscences,' especially chap. 13 ;

Perhaps the greatest result was his demonstration, after a controversy extending over a quarter of a century, that the Sigillarian and Calamarian trees of the Carboniferous period were Cryptogams. To use his own words: "The fight was always the same: Was Brongniart right or wrong, when he uttered his dogma, that if the stem of a fossil plant contained a secondary growth of wood, the product of a cambium layer, it could not possibly belong to the cryptogamic division of the vegetable kingdom?" Williamson ultimately succeeded in convincing his opponents, including almost all the members even of the French school, that the plants in question are nothing but highly organised Cryptogams, their secondary growth being mainly an adaptation to arborescent habit, and by no means an indication of Phanerogamic affinities. In this controversy Williamson had two sets of opponents; namely, those who followed Brongniart in regarding plants with secondary growth as necessarily phanerogamic, and those who, while recognising the cryptogamic nature of the plants under discussion, denied or minimised the secondary growth itself. Williamson, in spite of occasional mistakes in detail, was ultimately victorious on both issues; there is to-day, not the slightest doubt that most Palæozoic Cryptogams formed, by means of cambium, secondary tissues essentially similar to those of Dicotyledons or Gymnosperms, and that these plants were none the less as truly cryptogamic as their less highly organised representatives at the present day.

But, apart from this controversy, upon which it is superfluous to dwell longer, Williamson advanced our knowledge of the ancient plants in many directions, especially as regards the Sphenophyllæ, of which he discovered the first fructifications showing structure; the fructifications of Calamariæ and Lepidodendreæ; the various types of structure among the fossil Lycopods; the existence of a group on the frontier of Ferns and Cycads, &c. He made mistakes, as all do, who carry out extensive investigations in a new field, but he corrected most of them himself, and they in no way affect the permanent value of his great work in laying the secure foundations of scientific palæozoic botany.

Williamson's remarkable skill as a draughtsman added greatly to the value of his memoirs, which are illustrated almost wholly by his own hand. He was by nature an artist, and, in addition to his scientific drawings, painted many pleasing landscapes in water-colours during his leisure hours.

Williamson was an all-round naturalist of a type now unhappily all but extinct. He made his mark as a distinguished original the obituary notice by Solms-Laubach, in 'Nature' for September 5, 1895; and D. H. Scott, "Williamson's Researches on the Carboniferous Flora," 'Science Progress,' December, 1895.

investigator in three distinct sciences; in geology, by his early work on zonal distribution of the fossils on the Yorkshire coast, and again by his investigations of the Foraminifera of marine deposits; in zoology, by his researches on the development of the teeth and bones, not to mention his work on recent Foraminifera and Rotifera; in botany, by his elucidation of the structure of fossil plants. It would be difficult to find another example from our own time of equally varied and successful scientific activity.

His ability was recognised by competent men of science from his early youth upwards, and during all the earlier part of his career his work was of an advanced type, and up to the best standard of the day. At a later period, especially during his investigations of the Carboniferous Flora, this was no longer the case in an equal degree. Owing chiefly, perhaps, to his want of knowledge of German, his later publications suffered somewhat from his insufficient familiarity with the results of modern botanical work, and with the consequent technical expressions. This makes some of his writings hard to follow, and has led to their being estimated below their true value by some botanists of a more modern school, who have sometimes failed to appreciate discoveries, however important, unless recorded in the current vernacular of modern science. Those, however, who take the trouble to surmount this initial difficulty, will always be astonished at the wealth of observation which his work contains, and at the sound judgment which he brought to bear on his discoveries.

After his retirement from official duties in 1892, Williamson spent the last three years of his life near London in peaceful devotion to his favourite studies, continuing his scientific researches to the last. His death took place at his house at Clapham Common, on June 23, 1895, at the age of 78.

His unique collection of slides, illustrating the microscopical structure of fossil plants, has happily been acquired by the British Museum (Natural History Department).

Williamson received various marks of public recognition during his long career. A Royal medal was awarded to him in 1874 for his researches on fossil plants, at a time when he had only published six out of his nineteen memoirs in the 'Philosophical Transactions'; in 1890 he received the Wollaston medal of the Geological Society; he was a foreign member of the Göttingen Academy of Sciences, and of the Royal Society of Sweden; in 1883, the University of Edinburgh conferred upon him the degree of LL.D.

D. H. S.

Admiral Sir GEORGE HENRY RICHARDS, K.C.B. This officer, the son of Captain G. S. Richards, R.N., was born in 1820, and entered the Royal Navy, on board the "Rhodamanthus," in 1833, and served

in her in the West Indies for two years under the late Admiral G. Graves. In 1835 he was appointed midshipman in an expedition consisting of the "Sulphur" and "Starling," fitting out under the late Admiral F. W. Beechey, for exploration and survey in the Pacific. He served for five years in the "Sulphur," chiefly under Sir Edward Belcher, on the surveys of the West Coasts of South and North America, the Pacific Islands, New Guinea, and the Moluccas, and was then transferred as Senior Executive Officer to the "Starling," Captain Kellett. He was present in her during the first Chinese War at the taking of the Bogue forts and the capture of Canton. The ship returned to England in 1842.

After three months in the "Caledonia," under the flag of Sir David Milne, he was, on July 12, 1842, promoted to Lieutenant, and appointed to the "Philomel," fitting for the survey of the Falkland Islands, under Captain Bartholomew Sullivan. The "Philomel" was, however, diverted from this survey to take part in the operations against Rosas, the President of the Republic of Buenos Ayres, in 1845-46. Lieutenant Richards was present at the different actions in the Parana and the Uruguay, and commanded the boats of the "Philomel" at the cutting out of a schooner at night under a heavy fire of musketry from the banks of the Uruguay, and received the thanks of the senior officer, Sir C. Hotham, on the quarter deck of the "Gorgon."

He was senior lieutenant at the attack of the forts at Obligado in the Parana on November 18, 1845, and commanded the small-arm men of the "Philomel" at the storming of the batteries and capture of the guns which were taken on board the ships. On his return to England, in June, 1846, he was promoted to Commander from the date of the action.

In 1847 he was appointed to the "Acheron," Captain J. Lort Stokes, destined for the survey of New Zealand, and was employed for four years on this service. The existing charts of this colony are mainly the result of this survey.

Returning home, in 1852, Commander Richards volunteered for, and was immediately appointed to, an expedition fitting out for the Arctic Regions to continue the search for the missing ships of Sir John Franklin, and in April of that year sailed as Commander of the "Assistance," and second to Sir Edward Belcher in the Wellington Channel division of the squadron.

Whilst on this service he conducted several extended sledging expeditions, travelling more than 2,000 miles over the frozen sea, mapping many unknown coasts, and being absent from the ships on such duty for a period of, on the whole, seven months. Commander Richards' unvarying good humour and good fellowship did much to render this expedition a success under very trying circumstances.

On his return to England in the autumn of 1854 he was promoted to the rank of Captain, and was not again employed till 1856, when he was appointed to the command of the "Plumper," in charge of the survey of Vancouver Island and the coasts of British Columbia. He was at the same time nominated a Queen's Commissioner conjointly with Captain Prevost, R.N., for settling the Oregon boundary question between Great Britain and the United States.

Captain Richards settled the point on the coast from which the boundary line should start, and rendered efficient aid to the combined party of Royal Engineers and others who traced it to the eastward.

In the "Plumper," and subsequently in the "Hecate," he conducted for seven years the surveys of the intricate and rock-studded coasts and channels of Vancouver and British Columbia, accomplishing a marvellous amount of work. He returned to England in 1863 by the islands of the Western Pacific, Australia, and Torres Straits, making surveys and fixing longitudes on the way. This voyage completed his third circumnavigation of the globe.

He arrived in England to find himself appointed Hydrographer of the Admiralty, the late occupant of the post, Admiral Washington, having recently died.

Captain Richards held this post for 10 years, and by his powers of organisation, and the most unremitting industry, greatly increased the efficiency of his department, which he administered with great skill, and placed upon a firm basis to meet its ever growing work.

A new scheme of retirement placed Richards, who had attained the rank of Rear-Admiral on June 2, 1870, on the retired list in 1874, when he left the Admiralty.

Whilst Hydrographer he did all in his power to further scientific exploration of the sea. The preliminary voyages made by Dr. Carpenter, Mr. Gwyn Jeffreys, and Dr. Wyville Thomson in the "Porcupine," "Lightning," and other of H.M. surveying vessels in 1868-71 were promoted by him, and led up to the ever memorable expedition of the "Challenger" in 1872, in the inception of which he played a very important part, whilst its fitting out and organisation were carried out under his superintendence.

He also made the preliminary arrangements for the transport of the expeditions for the observation of the Transit of Venus in 1874, which were carried out shortly after he relinquished office.

In 1866 Richards was elected a Fellow of the Royal Society, and in the same year a Corresponding Member of the Academy of Sciences of Paris. He was also an active member of the Royal Geographical Society, serving on the Council.

In 1869 he was nominated an A.D.C. to the Queen, and in 1871 a Companion of the Bath. He received the honour of knighthood in

1877, and in 1888 the Knight Commandership of the Military Division of the Bath.

Admiral Richards was, while serving at the Admiralty and subsequently, a trusted adviser of several administrations, and was a member of several committees on confidential and general subjects, and was also President of the Arctic Committee which sat in 1875.

He became a Vice-Admiral in 1877, and Admiral in 1884.

After leaving the Admiralty he was at once offered and accepted the position of Managing Director of the Telegraph Construction and Maintenance Company, which he held for twenty years, when he was elected Chairman of the Company, a post he occupied to his death.

Whilst Managing Director, some 76,000 miles of submarine cables were laid under his superintendence in different parts of the world.

He was also Acting Conservator of the Mersey from the year 1888, an important post in connection with the well-being of that great seaport.

Sir George Richards served several times on the Council of the Royal Society, and was nominated a Vice-President.

He was a man of great ability, of sound common-sense, and of untiring activity, and his unfailing good humour, general shrewdness, and kindness to younger members of his profession caused him to be universally beloved and respected.

He died at Bath on November 14, 1896, somewhat suddenly, though after a painful period of severe sciatica.

Sir G. Richards married, first, in 1847, Mary, a daughter of Captain R. Young, R.E., by whom he had several sons and daughters; and, secondly, Alice Mary, daughter of the Rev. R. S. Tabor, of Cheam, who survives him.

W. J. L. W.



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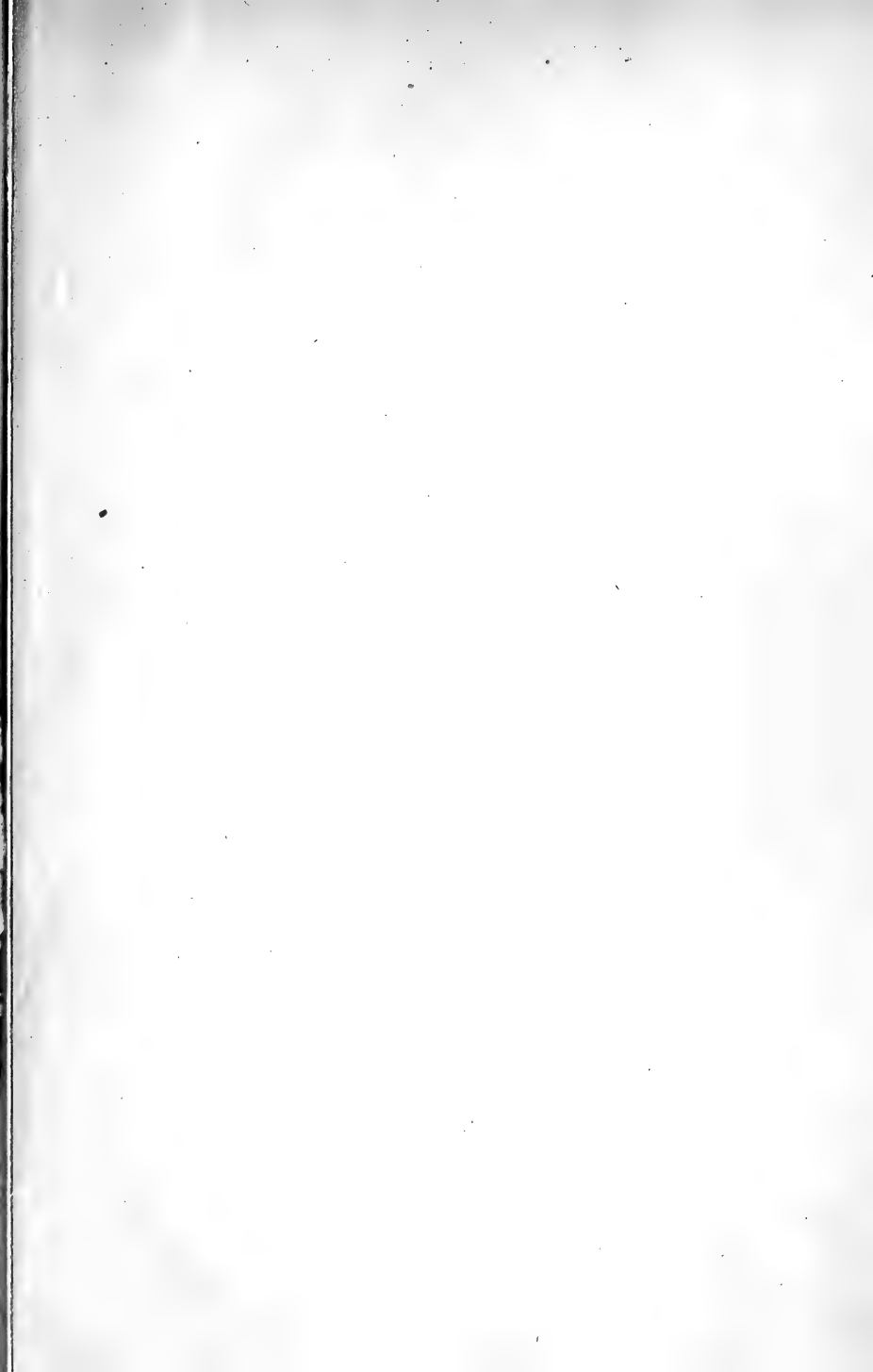
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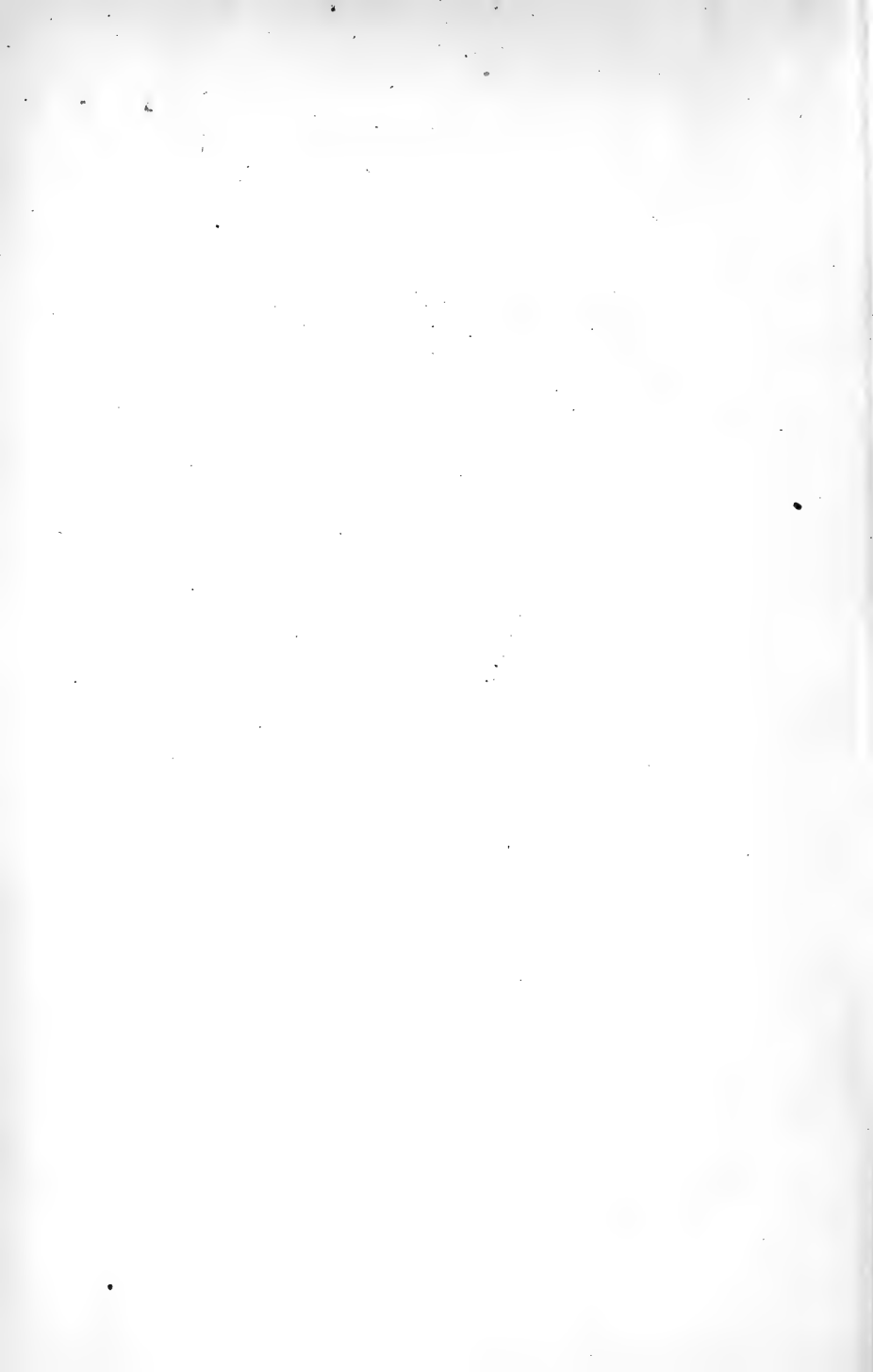
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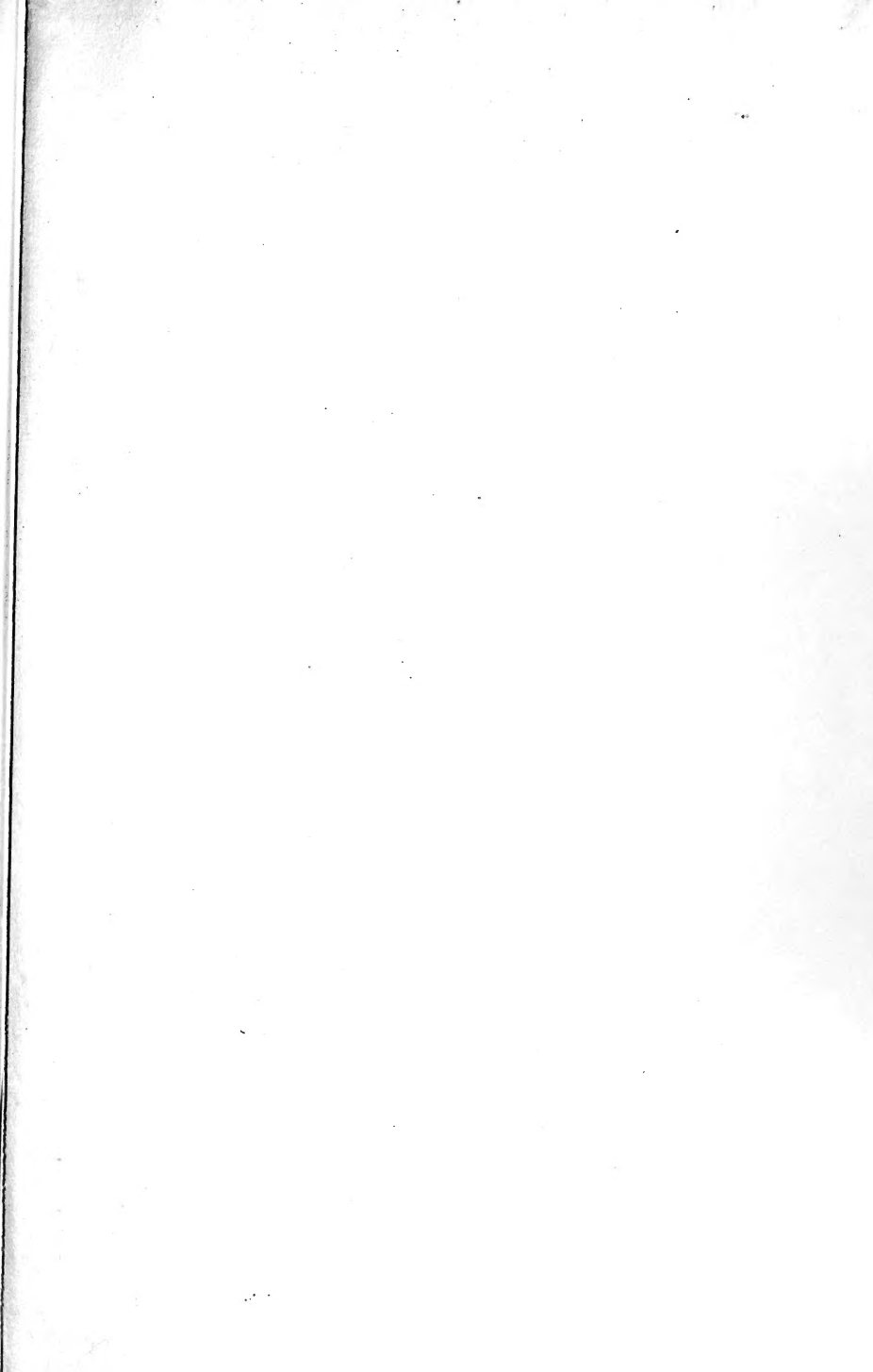
ERRATUM.

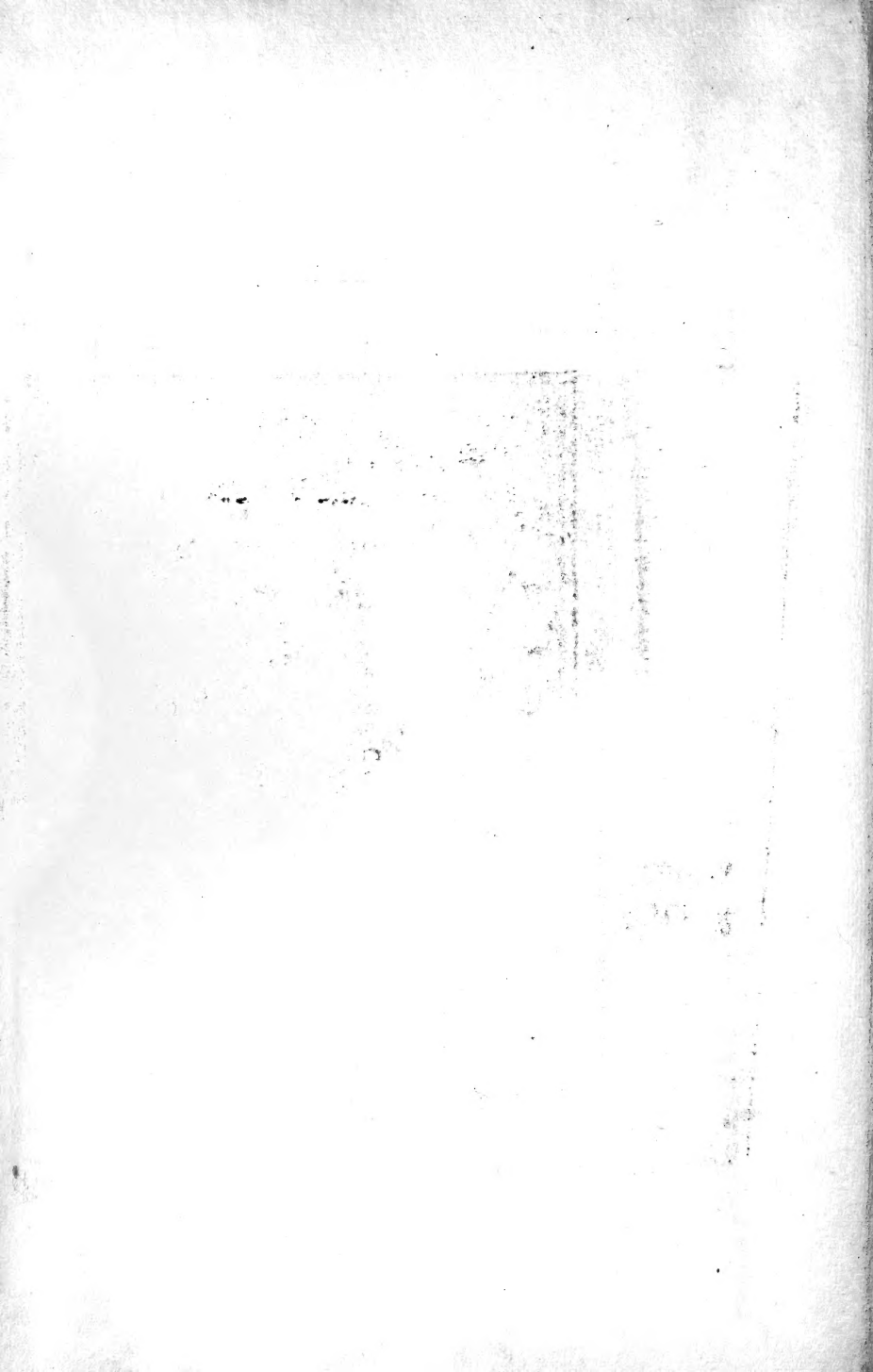
P. 313, line 23. For Philip P. Lenard, read Philipp Lenard.

THE END.









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